Antiquity

A Quarterly Review of Archaeology

VOL. VII NO. 25 MARCH 1933

Editorial Notes

PLANNING is naturally repugnant to many people, in the realm of action as well as of thought (assuming such realms to exist and to be separable). It is therefore not altogether surprising that there should still be no adequate map of the Roman Empire, none indeed so informative as the graphic representation of it compiled in the 6th century and now in the National Library at Vienna. The reason is not far to seek ; there is a great gulf fixed between the historian and the cartographer. The historian deals with the written word, which is both the subject matter of his researches and the vehicle that conveys his discoveries to the world at large. He resides as a rule in Universities and has little or no contact with the makers of maps. Cartographers, on the other hand, are paid to produce maps for the use of soldiers, motorists, hikers, and others who use them to find their way about. The technique of map-production is a closed and uncut book to most historians ; and the realm of history is one that the cartographer seldom has the time or even the desire to explore. Between the two professions, however, there is of course no antagonism ; there is merely lack of contact, and perhaps of any recognition that such contact might be mutually profitable.

It was with some knowledge of these facts that the present writer proposed in 1928 the compilation, by international collaboration, of a

Map of the Roman Empire on a scale of 1 : 1,000,000 (about 16 miles to the inch). The basis for the publication of such a map existed in the shape of the International Map of the World on the same scale, though it is significant of the gulf referred to above that no one of the historians and archaeologists now compiling this (Roman) map had ever heard of the International Map. (For this state of affairs the cartographers must surely accept some of the responsibility). So far as the area of the Roman Empire is concerned the International $\frac{1}{M}$ map is already available, only a few of the 52 sheets (and those not important ones) still remaining unpublished.

The proposal met with a good response, especially in Italy ; and as the outcome of further conferences a definite programme of work was adopted. The organization was at first in the hands of the International Geographical Union, at whose Congress in Cambridge the proposal was made. In 1931, however, the scheme was entrusted to the Central Bureau of the International $\frac{1}{M}$ map, whose headquarters are located at Southampton, at the Ordnance Survey of Great Britain. In 1932 the President of the Bureau, Brigadier Winterbotham, received an invitation from the Italian Government to attend a conference of all those who had hitherto participated in the scheme. The conference took place in Rome in November last and was generally agreed to be a great success.

Representatives of Italy, Great Britain, France, Germany, Spain and Portugal were present and representatives of other countries invited were only prevented from attending by reasons of a professional or geographical character. At the formal meetings the practical difficulties encountered by the compilers of the map were pooled and discussed, and the suggestions made were voted upon and adopted, either unanimously or (in a few minor instances) by a majority vote. There was one matter which was agreed upon without discussion and with unanimous enthusiasm, and that was the character of the welcome extended to the conference by the Italian Government. Everyone conspired to make the visit both enjoyable and profitable.

Excursions were organized to the principal archaeological sites—in Rome itself, at Ostia and at Pompeii and Herculaneum, and it was agreed by all members of the conference that Italy is now an archaeologists' paradise.

Specimens of the seven sheets of the map already printed were presented ; these will now be published and put on sale at an early date. (The maps can be obtained from official agents in each country, the agent in Great Britain being Mr Edward Stanford, Long Acre, London, W.C. 1). After the conference one of the British representatives visited Vienna, Budapest and Basel and obtained the collaboration of archaeologists and historians in Austria, Hungary and Switzerland. It should further be stated that the Egyptian Government is actively cooperating and hopes shortly to produce a sheet of the map. The scheme has therefore passed from the realm of abstract ideas to that of a concrete undertaking ; it is producing what it was intended to produce ; and there is no reason why it should not be carried to a successful conclusion within a reasonable lapse of time, provided it is not thwarted by events in the outside world of affairs.

A correspondent has asked us to express an opinion about the 'proper function of local societies', using the word 'local' in contrast to ' national '. As a matter of fact he gave one himself with which we are in complete agreement, namely, that it consists in the ' collection and publication of *facts* without attendant masses of verbiage which attempt the collation of all the various aspects of a given problem '. It is not, for instance, necessary to discuss the whole of the British Bronze Age when the discovery of a bronze axe is recorded in the Transactions of the Stilton and South Lincolnshire Natural History and Philosophical Society, to take an imaginary example. Such discussions do not arise on such occasions and should be firmly ruled out by the Editor. They make it more difficult for the reader to discover the facts, which is what he wants. Irrelevant theorizing has been the besetting sin of the local archaeologist from the earliest times, and it is time it stopped.

Another correspondent suggests a short article on the ethics of publication. ' For instance, should mention be made of any object in a museum which is open to the public without the permission of the finder ? It stands to reason, unless the object has been in the museum for a long time, that it should not be photographed nor drawn, but how long should the interval be before mention of it becomes public property ? As a rule, objects kept by a State Museum are immediately put on view before the report on them has been issued to the excavator, which seems to me to be rather hard on the latter'. Here is a *practical* problem of ethics for the students and professors of Moral Philosophy to solve. We leave it there, adding however that we cannot undertake to answer, or even acknowledge, letters we may receive on the subject.

We have received many agreeable communications with reference to our ' domestic ' notes in the December number and we wish to thank all those who have already so promptly sent their subscriptions for 1933. On several occasions we have pointed out that spontaneous payment is a very great help and we would ask attention to the notice printed below.

The SUBSCRIPTION to ANTIQUITY for 1933 is now DUE. We would remind our Subscribers of the form and envelope inserted in the December number for the purpose of remitting payments. *An early response will be much appreciated as this will save avoidable trouble in having to send out direct reminders.*

Payment should be made to

ANTIQUITY, 24 *Parkend Road, Gloucester.*

Archaeology as a Science

by David Randall-MacIver

[Dr Randall-MacIver's presidential address on 'The Place of Archaeology as a Science', read to Section H of the British Association at York in 1931, is so important and of such general interest that we sought his permission to print an abstract. The Address, which presents in a very acceptable way the scientific aspects of Archaeology, deserves to be widely read and it is hoped that through our pages it will reach many who would not otherwise see it, even though it has, owing to the length of the complete text, to be given in an abbreviated form. The full address is published in 'The Progress of Science' and also in the Report of the British Association for 1932, pp. 147–68. In Nature, 24 September, 1932, is a fuller version of the 'Canons of Archaeological Theory' than we have been able to print here, and in its issue for 10 September, is a discussion by a noted authority on some aspects of public policy put forward by Dr Randall-MacIver.—Editor.]

ARCHAEOLOGY, like anthropology, is a very young science, and like anthropology it has grown at a most astonishing rate. In the true sense it is scarcely a hundred years old, for its birth may be placed about the middle of the last century, unless we are willing to give a rather artificial value to that false dawn which came with the occupation of Egypt by Napoleon. I should rather prefer to say that it begins just about 1850. Layard was excavating at Nineveh in 1845. Boucher de Perthes published his first work on stone implements in 1841 ; and the entire theory was made known in England in 1858, in the same year that Darwin and Wallace read 'On the Origin of Species'. Keller's work on lake-dwellings appeared in 1854. Lartet and Christy were doing their chief work in 1861, and Pigorini from 1862 onwards. Schliemann's excavations of Troy began in 1870.

Inasmuch as it is a study of man and his works, archaeology is very closely related to anthropology. What then, we may ask, is the precise character of this alliance ? Each deals with man and nothing but man, but from different points of view, so that the two sciences are supplementary to one another. Obviously anthropology is the wider of the two, for it treats not only of man's material works but also of his mental, moral and sociological development. Anthropology

moreover totally disregards date and time, it simply studies primitive man wherever and whenever he is found ; and primitive man may exist and does exist in the twentieth century A.D. as well as in many thousands of years before Christ ; though he has become rarer in these later days and is not so widely distributed over the earth. Strictly speaking, both civilized and uncivilized man should fall equally within the range of anthropology, which claims to be nothing less than the study of all mankind in every relation. But the latest and more complex developments of civilization which are manifest in our own day have been appropriated by younger and more specialized sciences such as sociology and psychology, so that except for an almost academic distinction it may be said that anthropology confines itself to *primitive* man. It has two distinct branches—the one which examines man simply as an animal, the other which studies him as a *rational* animal. With the first of these, termed physical anthropology, which is really a branch of zoology, our science has very little to do. It may accept and use its results occasionally as a background, but with the same detachment that it shows towards zoology or geology. For the interest of archaeology is solely in those works which can only be produced by man when he has become more or less *sapiens*. Even ethnology, which is physical anthropology as applied to the developed races of man, has only a very slight and limited usefulness for the archaeologist.

From those branches of anthropology, on the other hand, which reveal man in his religious, sociological and cultural relations, and those which study his arts and crafts, archaeology derives the whole of its theoretical structure. How intimately the two subjects are related is shown by such a book as Sollas's *Ancient Hunters*, in which, if it were not for the headings of the chapters, the reader could hardly tell at any given moment whether it is an ancient or a modern people that is being described. Without anthropology, in fact, archaeology would be blind of one eye and very short-sighted of the other. For the only possible subject of archaeology is the *material* output of man, the visible products of his hands, whether these are shown in agriculture, building and other modifications of the surrounding world, or in those manufactures, arts and crafts by which man improves the conditions and amenities of his material existence. What man has been thinking or feeling, or just *why* he did any of the things that we find him doing, archaeology can never directly ascertain. What it discovers is merely the bare fact ; it can never divine the essence of the fact, that which gives it all its meaning and its interest. For the whole interpretation

of the inner meaning and rationale of man's life we are necessarily dependent either on anthropology or on history—that is to say, on records and observations of the thought, habits and behaviour of men who could be actually studied as living and thinking beings. Without the aid of these records archaeology would indeed be a musty science ; but when it employs them it is able inferentially and by analogy to construct the whole of man's story from his earliest beginnings to the present day. And this reconstruction is not only a book, it is an illustrated picture-book, richer than mere anthropology and richer than mere history.

Of the two auxiliary sciences, anthropology and history, the former is generally more useful to us, just because it deals with the primitive, and ancient man is necessarily more or less primitive. Documentary history is very limited in its range ; it gives only a few glimpses of the life of ancient times, and covers only a very small section of the immense period over which archaeology must range. Occasionally, however, it throws a vivid searchlight on times which are especially interesting to us as being comparatively near our own, and usefully supplements our anthropological knowledge by information as to what civilized people, as distinct from savages, thought, felt, and said. Its principal and indispensable function, however, is that of providing a time-scale, which cannot be obtained from any other source, even though its time-scale only covers a few thousand years. A few thousand years is only a small fraction of the time which is included in archaeology. The material of this science goes back to the Tertiary period in geology, innumerable thousands of years before the first beginnings of writing or the first whisperings of tradition. It begins even earlier than zoological anthropology, for in the Chellean, not to speak of pre-Chellean, flints we have records of man's handiwork which antedate any actually known remains of man himself. For these immeasurably remote periods a very rough and inaccurate time-scale, which is, however, steadily being improved, has been provided by geology. It is not until about 3,500 years before Christ that this clumsy instrument can be replaced by a much finer one derived from inscriptions and documentary evidence. Then comes a stage of overlap when the interaction of historical tradition and archaeological study is extraordinarily fertile. At this stage we are able to build on our most solid foundations, when archaeology synchronizes with written records or with the epics, sagas and genealogies which precede them. This, if we care to make this distinction, is the period of proto-history as

distinct from pre-history. It is the time which is most familiar to the general public, and naturally the most attractive. For it illustrates the dawn of all those great civilizations, oriental and classical, which enter into the intellectual life and interests of all cultivated people. Egypt, Elam and Sumeria, the Crete of Minos, and the Troy and Mycenae of Homer are some of the subjects of this period.

But archaeology does not end where history begins ; it does not even end when written histories are numerous and fully documented. All through the classical periods of Greece and Rome, and all through the Middle Ages, history needs and receives the greatest assistance from archaeology. Down to at least A.D. 1000 archaeology is needed as much as documentary evidence for reconstructing the life of any people. It is not until written records of every kind are so minute in character and so abundant in quantity as to cover almost the whole field of life that archaeology becomes superfluous. Then gradually it gives way, but does not wholly cease to exist until all ' old things ' have been replaced by new and modern things, which is almost the time of our own generation.

The organization of archaeology may be treated under three headings. First there is the collection of the material in the field and the recording of it. Secondly there is the housing, conservation and exhibition of this material in museums. Thirdly there is the comparative study of all such material, and the digesting and dissemination of the results in books of synthesis and popularization. Each of these activities demands separate consideration.

The collection of material in the first instance is due to the work of the explorer. He may either travel through a country observing its visible features and monuments, or he may seek to discover new material by excavating what has been hidden underground, either deliberately in tombs and treasuries or accidentally by the accumulation of sand and soil over the deserted ruins of ancient buildings. At the present moment our chief attention is centred on excavation and our most sensational results are being obtained thereby. Recent excavations in Egypt, Mesopotamia, Greece and India are of vivid interest to every cultivated person. As one who has excavated for a good many years and has had constant opportunities of studying all aspects of the excavator's problems, I have been able to form some very clear ideas as to the policy and general necessities of science in this regard. First there are one or two elementary axioms, which were once generally ignored, but are now so universally recognized that they need only be

mentioned and emphasized as axioms. The most important of these is that no person who is not qualified by special knowledge and study should ever be allowed to excavate at all. And since individuals are not impartial judges of their own capacity, this comes to mean that no one must excavate unless he is endorsed by a scientific institution or at least by a committee of scientific men. This necessity is explicitly recognized almost everywhere, though I can remember some flagrant instances of the violation of the rule even in these last few years. It is a rule, however, which can admit of no exceptions. The days are long past when the looting of sites for the amusement or personal profit of a private individual could be tolerated, and no government with any pretensions to enlightenment will ever again allow it. But various countries which have only recently arrived at autonomy may need warning in this respect, and it would be well that public opinion should be fully alive to the danger. Powerful interests, both individual and political, are often enrolled against our science, and we may sometimes regret that there is no scientific League of Nations to which we might appeal.

Let us consider what happens and what ought to happen when a museum, a university, or a scientific body of any kind sends out its duly qualified explorer. Both explorer and employers have certain perfectly clear duties to discharge, and I suggest that on one side these are not sufficiently recognized. It is the explorer's business not only to furnish his home museum with collections of valuable specimens, but also to make the most complete study of all the conditions under which they are found and to publish this study in the fullest possible form. But here he is very often fettered by the unthinking or deliberately selfish egotism of his employers. A great deal of pressure is often brought to bear on the explorer to make him not only excavate the most lucrative sites, which may be quite legitimate, but to neglect the less attractive and remunerative parts of his concession. This is so notorious that I need not quote instances. I prefer rather to recall the admirable public spirit shown by a great American institution, which ungrudgingly and uncomplainingly supported its representative through several years of expensive and quite unremunerative trenching which he judged to be necessary. And it is pleasant to relate that this generosity was rewarded by the eventual discovery of prizes which excelled their wildest dreams.

If it is the explorer's duty fully to study and record whatever he finds, it is a duty which is never neglected in these enlightened days by

any scientist at all worthy of the name. But it is not quite so invariably a part of his creed that the privilege of exploration carries with it the implied promise to publish, and to publish quickly and fully. The record of British archaeologists is very honourable in this respect, and there is hardly any important field-work that has not been published or is not in process of publication. A great deal of credit for this happy state of things must be given to the doyen of British excavators, Sir Flinders Petrie, who has never failed, in spite of every obstacle, to furnish a published account of his field-work within the shortest possible time of its completion. This example and the growing pressure of public opinion have been very effective in Great Britain, but several continental countries have fallen far short of our standard. It is sometimes the fault of the individual, sometimes of the institution. There are some men who direct workmen admirably, but seem to be seized with paralysis at any mention of publication. And there are many institutions which make no provision and take no thought for the publication of their material, once it has been safely hoarded in their exhibition rooms. Now let me be perfectly outspoken on this matter. That explorations should be made and left unpublished is a disaster, and if the explorer or his employers are responsible for this failure it is a crime. Nothing can take the place of publication. Notes, drawings, photographs and plans, however elaborate and careful, are of very limited usefulness except to the man who made them and who can alone interpret them. It is an error even to suppose that a literary executor can take over the material and produce a satisfactory result. If an archaeologist does not bring out his material, or at least fully prepare it for publication, in his own lifetime, a great part of it is irretrievably lost to the world.

This being the case, the institution which obtains a site for excavation ought to guarantee the expenses of a reasonable publication and ought to bind its excavator by contract to publish. I may quote as an example my personal experience with an institution which appreciated its duties fully and exactly. In January of 1907 I accepted a contract with the University of Pennsylvania to conduct excavations in Egypt and northern Sudan. The term was fixed at five years, and the University stipulated that before the lapse of these five years I should have prepared for publication a full report of all the results. In accepting this provision I stipulated on my side that the University should publish every word that I might write and every illustration that I might deem necessary. No obstacles were allowed to stand in

the way, and the contract was precisely fulfilled on both sides. I consider that such an undertaking ought to be given by every institution that sends its man into the field, and that this should be so fully recognized that the excavator need not even have to propose it.

There is still one more consideration in regard to the exploration of sites which is very little appreciated. The wisdom of one generation, even if it be our own, is inadequate to foresee all possible problems. Therefore, whenever the circumstances allow, a portion of every site should be left unexplored and reserved for future study. The advantages of this are manifest ; let me quote only two examples. The results obtained at Pompeii within the last ten years have been so revolutionary that they have put all the old standard books out of date. If this city had been cleared at one sweep when first discovered all this knowledge would have been lost, owing to the imperfections of the methods then in use. The proper technique has only gradually been evolved. On the other hand, the frantic rush to explore all lake-dwellings in the third quarter of the last century barely left Vouga enough material for the studies which he has just completed. Had they been *all* destroyed by the first excavators the mistaken opinions launched seventy years ago would have been stereotyped for ever.

The second aspect of an archaeologist's activities is museum work. Sometimes the same man who has formed a collection in the field will be placed in charge of it in a museum. This is a very happy arrangement and ensures that the most minute and intelligent attention will be given to everything that has been found. More often, however, the museum curator is a person who stays at home, and acts as the recipient and custodian of the collections that are brought to him.

How he treats these collections must be to a great extent determined by the circumstances and the accommodation at his disposal. Our greatest museums in England and on the Continent are in many instances so overcrowded, and so hampered by an excess of concentrated material, that it is useless to lay down ideal rules for them. The only hope for a really rational treatment of them is that they should be broken up into a number of smaller units ; this may for the moment be impracticable, but should certainly be borne in mind as the ideal at which any really systematic policy would aim.

In countries like Italy, with its traditional liberality towards science and art, or America, which starts in at a later stage with great resources and no hampering accumulation from past years, a genuinely systematic arrangement is possible. From the point of view of an excavator many

of the Italian museums are ideally arranged. The results of any given excavation are kept together in a single room, and each tomb and deposit is placed in a separate division of a case, carefully marked off from its neighbours. The effect of this is that a student can go into the museum at Florence or Bologna with the excavator's report in his hand, and study every paragraph with the objects in front of him. Even when the objects have been incompletely published I have been able to make a fairly systematic record of them from the mere contents of the cases thirty years after the work had been done. In the Egyptian department of the Metropolitan Museum at New York the deposits are not so rigidly kept in series—which is, indeed, difficult unless the available space is almost unlimited—but the same ideal has been borne in mind. The exhibition, therefore, can be used as an illustration of the actual excavation. Moreover, New York has gone far beyond any other institution in popularizing its exhibition. Photographs illustrating the stages of the excavation, abundant and detached labels and descriptions of the objects, and *résumés* of periods and styles of work make the collection an illustrated picture-book which has an immediate appeal for the public.

Many of our own museums might follow this example with advantage. A recent Government Commission, as you are aware, has published its reports on the museums in England. Amongst other things it remarks on the discouraging truth that the public does not seem to want museums. The same might be said of many places on the Continent. Now as conditions are at present, I must confess to having a good deal of sneaking sympathy with the public. A few institutions, like the British Museum and South Kensington, as well as a small number of enlightened provincial museums up and down the country, have published admirable handbooks, instituted popular lectures, and encouraged popular demonstrations by expert guides, yet these are only a very small minority. Whether in Great Britain or on the Continent the visitor to a museum, other than a gallery of pictures or sculpture, is merely left to drown in an uncharted sea of unintelligible cases. We can scarcely blame him if he objects to being drowned and rushes out into the fresh air. It is not the public but the management of the museum which is to blame. In America it would be quite untrue to say that the public does not want museums. On any holiday the Metropolitan in New York is crowded to overflowing by thousands of people, rich and poor, educated and uneducated, who show the most intelligent interest.

In order to popularize museums, however, a totally false start has been made in many places. With the mistaken idea that the ordinary man can appreciate art but cannot appreciate science, a number of institutions have been founded which are called Museums of Art and Science. The title might be allowed if it did not dictate the policy. But the policy has generally been to subordinate science, and presently almost to thrust it out of doors. The local magnate who has bought a few pictures for his own home, together with copies of the Apollo Belvedere and similar works which are supposed to be above criticism, declares to the committee of which he is chairman that the museum must not be filled up with old stones and pots and pans. And in the hope, very often unrealized, of a substantial legacy the committee obsequiously follows his lead. And very probably the director of the museum, who has been chosen for his talent as an art connoisseur, is very content with the policy of his committee. Now as far as the general public is concerned this is a sheer error of psychology. The ordinary man has no training and little aptitude for fine art, but he can understand workmanship, and he is interested in the things which come near either to his daily life, or to a life that he might have led some centuries ago. A well-illustrated and well-explained collection of ethnographical or archaeological objects makes a definite appeal to him, and he responds wonderfully to the romance of ancient history or of primitive life.

This supposed union of science and art is simply hypocritical, and when science has insinuated itself into a collection under the disguise of art it is high time that the disguise should be thrown off. A scientific collection is not made for aesthetic purposes ; it need not be ugly, and if capably handled it will not be ugly, but its primary purpose is not aesthetic. The attempt to aestheticize an archaeological collection is constantly being made, and always results in much damage to scientific interests and very little satisfaction to the aesthete. Let us be perfectly clear-sighted and frank about it. In itself archaeology has nothing to do with art—at most it chronicles the *history* of art ; which is a very different thing, as every artist knows, from genuine aesthetic appreciation. The art-critics are perfectly justified in protesting, as they constantly protest, against the confusion of art-history with art-criticism. The individual archaeologist may by the grace of heaven chance to be endowed, as a very few men are, with the real gift of aesthetic appreciation. But it is not directly evoked by his work, and there will be little opportunity as a rule for exercising it in the course

of his work. The immense majority of the objects with which he deals have very slight aesthetic worth; in so far as a man is purely archaeologist aesthetic values do not exist for him. The archaeologist works like a naturalist—it is his business to trace evolution, patterns, migration, and development; and when he is tempted to discourse on aesthetic values his opinions are very seldom worth hearing. Except in very rare instances, therefore, the products of excavation and exploration should be treated as natural history collections, and not as more or less unsuccessful efforts at pure art. And we must remember that archaeology has now happily become a popular subject. The man in the street is greatly interested in it. He delights in the pictures and the brief accounts which are published in the *Illustrated London News*; he rushes to the exhibitions of antiquities excavated at Ur of the Chaldees, or in Egypt, or anywhere else. The reporters of the most up-to-date American newspapers will assure you that archaeology is ' front-page news ', and it is printed with two-inch headlines in columns next to the exploits of the gangster and the gunman. This is fame—let us take advantage of it. It would be exceedingly foolish not to welcome this popularity and cultivate it by every possible means. Here is a study which does no harm to anyone, which any intelligent being can share, and which can add immensely to the amenity and happiness of the ordinary man's life.

I have now dealt with two aspects of an archaeologist's work, the collection of material and the exhibition of it in museums. The third is the dissemination of knowledge by means of books. Some of these books must necessarily be technical; others should be addressed less to specialists than to a cultivated public; and a third class ought to be directly and deliberately popular in their aim.

First of all, the original scientific accounts of excavations can hardly be popular works, and need not be. They are written for the professional and make very dry reading. They are not essentially literary in form, and if a writer inserts some chapters of literary character these are only an added grace; they are not essential at this first stage, but belong rather to the second. Lists, plans, schedules, catalogues and indexes are the fabric of which the excavator's reports ought to be composed. Their aim is to give a precise account of every feature of the exploration, and not until this has been done is there any occasion for general theories or estimates of the historical bearing of the discoveries. Books of this stage need be no more than mere chronicles; it is probably best that they should not attempt to be more. An

excavator need not be a literary man. If he has literary gifts he will have ample opportunity for using them in books of what I would call the second stage in the dissemination of archaeological knowledge.

For if it is the absolute duty of the excavator to produce a perfectly dry, passionless record of his work for the sake of his professional brethren, this is only the first stage in the process of bringing knowledge into general currency. When the seed has been thus gathered and sown it has to be watered and cultivated. This is a task which may be undertaken by the original explorer or by others. Unquestionably the best results are obtained when the explorer himself, if he has any literary ability, undertakes the popularization and exploitation of his own field work. No one else can so exactly estimate the finer values and all the different aspects of the discoveries which he has made. Indeed, any outside person will inevitably miss a great deal, and will probably view many details in a false perspective. Many of our best archaeologists have achieved as much success in semi-popular writing as in exploration ; I need only mention Sir Aurel Stein as a conspicuous example.

The writer of general synthetic works is often carried far beyond the possibilities of strictly logical proof. This does not mean that his methods are to be condemned. I fully realize the wisdom of a colleague who said to me many years ago, when we were discussing first principles on the banks of the Nile, ' You must not break archaeology on the syllogism '. It would be pedantry to ignore how much we owe to the poetic and far-seeing imagination of many a great archaeologist, from Schliemann down to some of our own contemporaries. The picturesque prophecy of today may well be the scientific fact of tomorrow. So long as the author keeps his fancies and his facts distinct, he can remain perfectly scientific. But it is his duty to show clearly the grounds of his reasoning ; and this leads me to consider somewhat tentatively what are the types of logical reasoning which may be regarded as conditionally or unconditionally valid.

From such a vast and intricate subject I will select for discussion only two of the principal problems of archaeology—namely, the application of a time-scale and the proof of the dissemination of a culture. First, then, as to the time-scale. A philosopher may attach little value to the mere arithmetical count of years, and a student will often work more freely if he thinks in culture periods rather than in centuries. But there is no doubt that the ordinary man demands not only ' facts ', but ' figures ', and it is a great temptation to supply the

figures at any cost. A series of culture-periods has been well established, so that there is a reliable system of what is called ' relative chronology ' from the earliest Stone Age down to the time of full documentary history. But it is a very different matter when we attempt to translate these culture-periods into centuries and thousands of years. The estimates given by various geologists and palaeontologists for everything behind the last stages of the Ice Age are immensely divergent from one another. I should not venture—it would be quite beyond my capacity—to criticize or discuss them. But when the stage of universal hunting has passed, and mankind has settled down to an agricultural and pastoral existence ; when the outlines of sea and land have become fixed in approximately the same forms which we know today, then we feel that prehistory is only a slight extension backward of what is generally recognized as simple history. It is a sketch of the early chapters in the story of empires, nations, and peoples, of whom several are known to us in written history or tradition. We naturally desire to know in terms of years and generations how far back we can trace the doings of the men who are our own ancestors or collateral forbears. Now here we must be clear-sighted enough to accept our inevitable limitations and avoid all sophistries and all claims, however specious, to know the unknowable. We are wholly dependent for our absolute chronology upon the dates recorded or obtained by immediate inference from ancient writings or traditions. The fragmentary relics of Mesopotamian and Egyptian official chronology furnish a time-scale, liable, as you know, to much uncertainty in minor details, but trustworthy in all its main lines. Whenever this time-scale can be applied, it is possible within quite narrow limits of variation to give precise figures as well as facts. Thus we can give a dating in years to all the products of Egyptian civilization back to the beginning of the First Dynasty. And by direct inference we can apply this scale to many other parts of Europe and Asia, as Sir Arthur Evans has so successfully applied it to the dating of Cretan civilization. Indeed, as archaeological discovery proceeds in the coming years we may reasonably hope to arrive at a completely graduated scale of chronological dating in actual years for every part of the ancient world after 3500 B.C. But if it is asked what means we have for establishing a chronological as well as a typological scheme behind 3500 or possibly 4000 B.C., I answer unhesitatingly that we have none, and that unless earlier written records or traditions come to light it is probable that we shall never have any.

One very crude method of attempting to avoid this impasse is so illogical that I need spend little time in discussing it. Below the strata in which definitely datable objects are found—whether at Knossos, Ur, Susa, Mohenjo-daro or any other very ancient site—there are generally strata of a certain thickness in which other and obviously earlier forms occur. Now it is sometimes suggested, even by skilled explorers in their less discreet moments, that the mere thickness of these undated layers may give an indication of the length of time which it took to form them. And yet a very slight amount of reflection, not to speak of actual experience in the field, will show that this reasoning is as childish as it is simple. I have myself seen in Egypt deposits many feet deep which can nevertheless be proved by well-dated objects at the top and bottom to have been formed within a single century ; and I have also seen a concentrated stratum of not more than four feet which contained the products of many centuries closely pressed together. There are innumerable reasons for which the rate of deposit may vary almost indefinitely. To attempt therefore to estimate the rate of deposit in the prehistoric stratum from that which is observed in the historical layers above it is worse than illegitimate, it is sheer fantasy.

In a less crude, but not very different form, the same error appears in the attempt made by several justly admired writers to establish a chronological scale for the typological series preceding the historical in a country like Egypt. The system of sequence-dating based on typology is now familiar to all students. It was established for Egypt by Sir Flinders Petrie, and for Europe in general by Montelius. As a scheme of relative chronology it sometimes creaks a little, but on the whole it works well and has justified itself, though it may need occasional emendation. But the recurring attempts made by one author after another to translate this relative system into an absolute chronology of years have no logical justification whatsoever and only encourage self-deception. The argument is really based on an assumption which can easily be shown to be fallacious. This is the assumption that the rate of progress in civilization is always uniform. If we know the rate of development in types which took place during the First and Second Dynasties and know also from inscriptions the length of these dynasties, then, it is argued, we have a yard-stick which can be applied to the period preceding the First and Second Dynasties. It is as though a policeman, having timed a speeding motor car over a measured mile, and found that it was going at sixty miles an hour, should appear before the magistrate and state that it was evident the defendant had been

proceeding all day at sixty miles an hour. The falsity of the conception is evident as a mere matter of logic ; but it can also be shown by numerous examples in the well-known periods of medieval and modern history. Would any historian, for instance, seriously maintain that the rate of intellectual and artistic achievement was exactly the same during the Dark Ages as in the Gothic time or the Renaissance ? Would anyone venture to argue that the industrial progress of the nineteenth century A.D. was no more rapid than that of the eighteenth, or that material development proceeded at the same rate in the reign of George I as in that of George V ? Merely to ask such questions is sufficient. I need not dwell on the long centuries of Byzantine or Chinese immobility, or on the static quality of much actual Egyptian art.

Next we may briefly consider the problem of the dissemination of cultures. This is one of the most interesting and important aspects of archaeological study. In it are involved all questions of the migration and movements of peoples, their commerce and intercourse of all kinds, and the degree and extent of their reciprocal influence upon one another. It is really the cardinal problem of archaeology, irresistibly attractive, and for that very reason offering peculiar temptations to hasty and premature generalization.

Now the foundations of this particular study, in so far as they have been well and truly laid at all, have been laid not by archaeology but by other sciences, those in fact which deal not with man himself but with the conditions necessary to his very existence. Geology, climatology, palaeontology, palaeobotany have been the instruments of that great progress in synthetic theory which I have pointed out as the special achievement of the last thirty or forty years. Those who have worked out the details and the stages of the Ice Age and the rainy periods have shown us that various parts of the world were uninhabitable for a long time. It is obvious, for instance, that man cannot exist under a snow-field, so that it is useless to look for him north of 50 degrees of latitude until the Ice Age is well past. That already reduces our problem to much smaller dimensions, and teaches us to exclude large parts of the world from the possible area of man's earliest evolution. Conversely, large areas which to the modern view seem impossible homes for man are shown to have been eminently suitable for the life of the palaeolithic hunter. The Sahara and the Gobi desert in their present condition cannot maintain the life of man or beast ; but the climatologist shows that there was a not very remote period when they were well-watered regions, covered with grass like the South African

veldt, and teeming with large game. Thus he explains what otherwise might have remained an ambiguous problem for the archaeologist, the finding of human implements of very early types in these apparently uninhabitable tracts.

The botanist next comes forward to tell us that the food plants on which a settled agricultural life depends can only be found in their wild state in certain closely defined areas. And he shows how changes of climate produce various types of afforestation which necessarily limit the movements and activities of a man who possesses only primitive tools. This type of reasoning has been exceedingly skilfully used, in particular by writers like Peake and Fleure, to restrict the range of choice and to give proportion, scale and limitation to the study of man's origin and movements. I regard this as one of the most solid achievements of recent years.

But when the archaeologist proceeds by purely archaeological methods to fill in the details on a background of which the outlines are thus immutably drawn by the other sciences, he is confronted with innumerable difficulties of method, and the logic of his procedure is not always well studied. In the first place we must necessarily rule out may types of reasoning which are so general and inconclusive that they can never carry any conviction. A little serious reflection must show that we necessarily know so little of the mental equipment of early man that it is often impossible to say what actions and habits are natural to all men as highly developed anthropoids, and what are so peculiar as to be specifically human and characteristic of one or another developed type of man.

To apply logic at all then, we need to find our material in highly specialized products or habits of man. In short, it is only possible to reason convincingly when manufactures or arts and crafts have reached a high point of intricacy.

In contrast to the doubts and uncertainties which beset all reasoning based on the manufactures and products of early man, it is a relief to turn to a field in which unquestionable logical certainty can be achieved. This is when we are able to study man's action in moving and displacing natural products. For when the natural distribution, as known to geologists, of rocks, ores, and other natural products is artificially changed there can be no doubt that man has been at work. The direction of his movements can be traced, the motive of his action can be divined, and even the intensity of his action can be measured. Thus if a certain kind of flint is peculiar to Grand Pressigny in France

and implements of that flint are found in Switzerland, there can be no doubt that Switzerland is trading with Pressigny. Similarly, if gold combined with antimony is known only to occur in Transylvania, it is a just, though a surprising, inference that the sceptre of a very early Egyptian king, living about 3000 B.C., which shows this unique combination of metals, is made of gold from Transylvania. To take a simple example from nearer home: if a number of stones in the circles of Stonehenge are of a type peculiar to Wales, they must have been transferred from Pembrokeshire to Salisbury Plain by man.

Raw materials, then, are better evidence than manufactures, especially in the earlier stages of man's life. When we are dealing with the works of man, logical processes of real value only begin to be applicable as handicrafts become more complicated and as the arts begin to emerge.

Belgic Cities of Britain

by R. E. M. Wheeler

I. The Belgae

WHAT particular gadfly drove the Belgae into Britain in the early years of the first century B.C. is hidden from us. The pestiferous Cimbri may have lent their sting; for although, alone of the Gauls, the Belgae had beaten them off along the Seine valley in 103 B.C., these vagabond Teutons and their friends cannot have added to the amenities of a continental existence. Thereafter, at any rate, Belgic ambition turned easily northwards to the familiar and relatively empty coasts of southeastern Britain. With a combined initiative and deliberation that may be imagined to have reflected their mixed Celtic and Teutonic origin, organized Belgic tribes or tribal contingents began to settle along our shores and to penetrate inland along our rivers. When Caesar arrived in 55 B.C., he found them, he tells us, in occupation of 'the maritime districts'.

Archaeologically, these Belgic pioneers have in some degree been known to us since 1890, when their curious, archaistic burial-urns, cordoned and pedestalled, were identified at Aylesford, on the Medway, by Arthur Evans.[1] But of their dwelling-places, their cities, their exploitation of the countryside, the spasmodic researches of a generation of students have taught us little. Verulamium and Colchester have of course been duly recognized as semi-historic Belgic capitals; but recent archaeology has failed to show that either of them is earlier than the last quarter of the first century B.C. At Hengistbury Head, flanking the natural harbourage of Christchurch in Hampshire, Belgae coasting westwards took their turn, perhaps at an early date, in the use of this convenient promontory. Post-Caesarian waves of Belgic immigration, of the late first century B.C. and early first century A.D., have indeed

[1] For the bibliography of the Belgic settlement of Gaul and Britain, see C. F. C. Hawkes and G. C. Dunning in *Archaeological Journal*, LXXXVII (1930), 150ff.; to which may be added Mrs B. H. Cunnington in *Antiquaries Journal*, XII (1932), 27ff., and C. F. C. Hawkes, *Ib.*, 411ff.

been postulated and localized by archaeologists in the more westerly regions of southern Britain. Ptolemy, in the second century A.D., even calls Bath a Belgic city, though with what implication we cannot say. These secondary phases of the problem do not concern us here. It is the main purpose of the present paper to attempt to throw a little more light upon the Belgae of Caesarian Britain, whose vaguely recorded exploits are the horizon of British history.

II. A Belgic Frontier

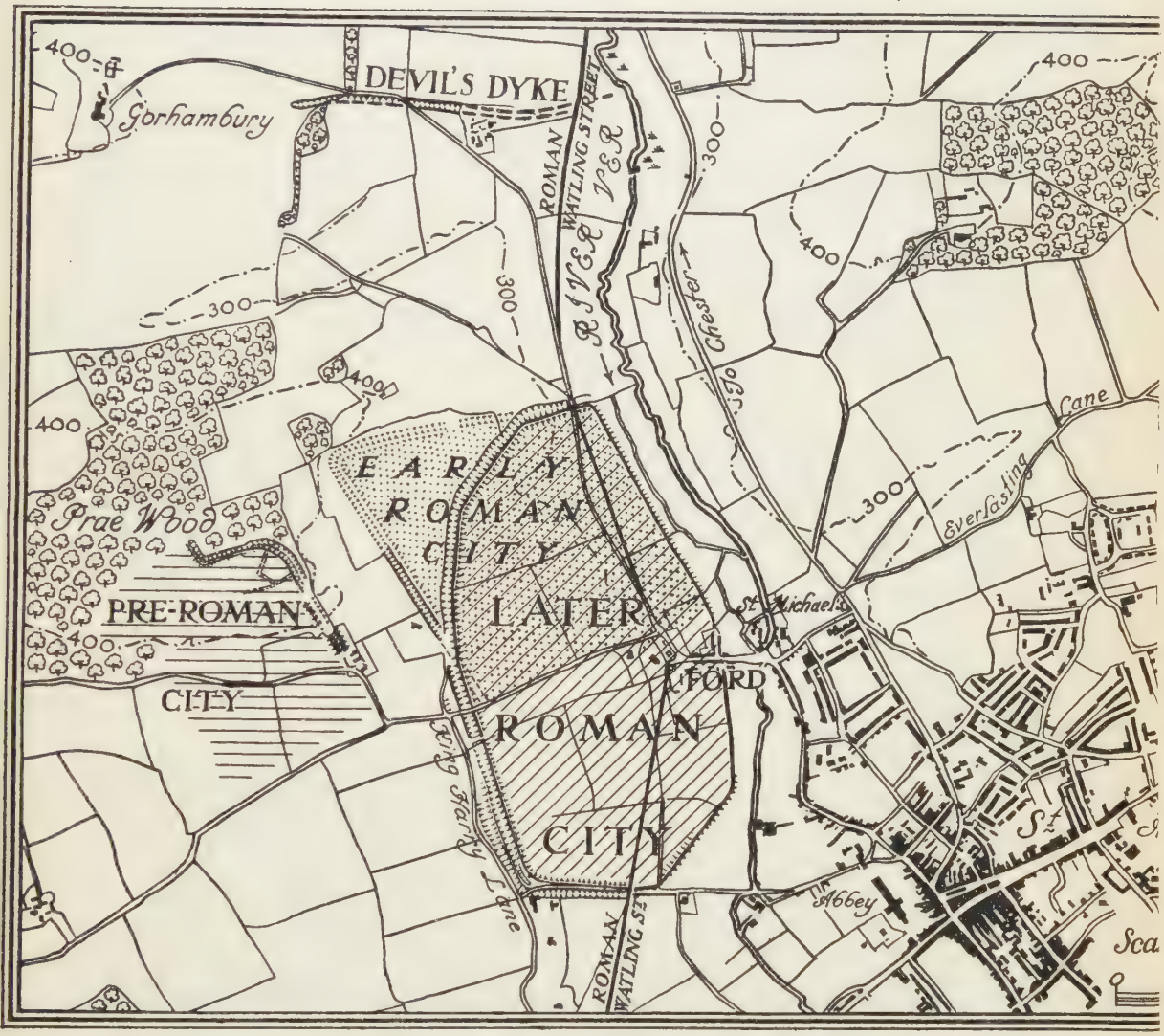
In July 1932, workmen digging a trench for a new sewer near St. Albans found a hoard of Roman coins. This occurrence was not in itself remarkable in a district where it is more difficult to avoid than to discover Roman remains. But investigation revealed attendant circumstances which at once gave to the hoard an entirely unusual significance, and that, curiously enough, in relation to the pre-Roman, not the Roman, era.

Unfortunately, the engineer in immediate charge of the work,[2] though cognizant of the discovery and ready subsequently to provide detailed and useful information, did not at the time see his duty clearly. The coins—' two shovels-full ' according to one account ; ' over a hundred ' according to another—were immediately dispersed, and only the most devious intrigues succeeded, after many days, in eliciting thirty-three of them for inspection. Most of these have now vanished again, but twenty-five of them were photographed before their return to their 'owners' (fig. 6).

The hoard, so far as recovered, is a simple enough matter. It consists of silver denarii, all marked with a distinctive copper stain, and ranges from several worn Republicans to an early Hadrian (A.D. 118–119). The last, though clipped, is almost in mint condition, and sufficiently indicates A.D. 120–130 as the approximate date of deposit.

The remarkable feature of the hoard lies in its situation. The spot (see map, fig. 1) is just over a mile north-northeast of St. Albans Abbey, on the southern side of a lane (Everlasting Lane), and at a point thirty feet west of the St. Albans–Harpenden road, opposite the southwestern end of ' Beech Bottom '. This name is applied to a formidable tree-clad ditch or dyke which occupies the bottom of a valley extending northeastwards from the Harpenden road to the

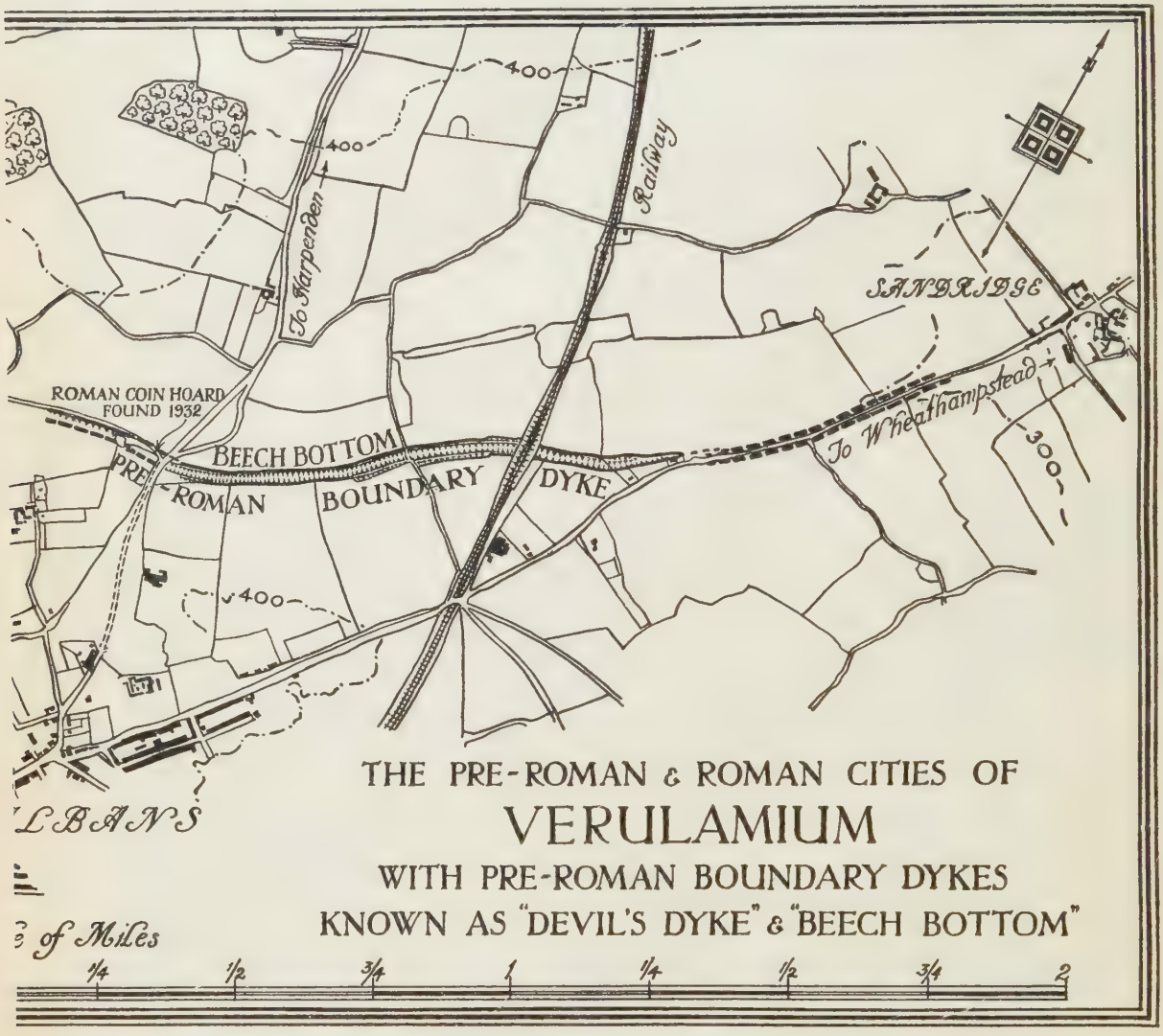
[2] It is fair to state that this work was carried out by a non-local firm.



FIG

(Based upon the Ordnance Survey 6-inch maps, Herts XXXIV N.W., N.E.

ΓE I



THE PRE-ROMAN & ROMAN CITIES OF
VERULAMIUM
WITH PRE-ROMAN BOUNDARY DYKES
KNOWN AS "DEVIL'S DYKE" & "BEECH BOTTOM"

and S.E., by permission of the Controller of H.M. Stationery Office.)

facing p. 22

vicinity of Sandridge. As a recognizable dyke, it is still open for a mile of this distance, and its northeastward continuation is marked by a sunken stretch of the St. Albans–Sandridge road. To the west of the Harpenden road its former line is represented, for at least a quarter of a mile along the valley, by the Everlasting Lane wherein the hoard was found. The scarp and northern bank of the levelled dyke can be traced faintly for that distance along the southern side of the lane.

At its eastern end the lane bends across the actual line of the dyke (see fig. 1), and the sewer, following the lane, here cut through the damp grey filling of the ditch, which had itself been excavated originally in a belt of compact orange gravel. The men were working at carefully measured depths, and it was at a depth of fourteen feet in the grey ditch-filling that the hoard was found. The sewer-trench at this point reached a final depth of seventeen feet, and at that depth was still in the filling.

The significance of these facts is at once apparent. Where best preserved, the Beech Bottom dyke is a hundred feet across and upwards of thirty feet deep (figs. 4 and 7). The hoard cannot have lain far from the centre-line of the dyke, and its depth from the present surface —only fourteen feet—implies that there were already ten feet or more of filling in the bottom of the dyke at this point before the hoard was dropped. In other words, *the dyke would appear to have been partly obliterated before A.D. 120–130. It was at least in existence by that date.*

From this a further inference follows. With its slavish adherence to the contour-line of the valley-bottom, the dyke is entirely foreign in conception to the practice of Roman engineering. True, the Roman Vallum behind Hadrian's Wall scorns command and often drops into lowly and obscure ground ; but the abject dependence of the Beech Bottom dyke upon every whim of the local contour is a different matter. It has nothing to do with Roman Britain of the first or second centuries A.D. And, since it is now proved to be not later than that period, we must conclude with certainty that it is earlier. It is pre-Roman.

So far, so good. The next problem is that of the function of the dyke. It was presumably in some sense a boundary. But of what ? Which way, in particular, did it face ? Here the position and structure of the dyke are neither of them very helpful.

First, as to position. The dyke follows the bottom of the shallow valley with undeviating impartiality ; it cannot therefore be attached by contour to one flank rather than to the other. Moreover, geologically it occupies almost exactly a narrow strip of dry valley-gravel

between symmetrical slopes of chalky subsoil with a patchy covering of boulder-clay. Its physiographical environment was therefore equally impartial (see fig. 2), and fails to enlighten the problem. Indeed it may at once be asserted that the coincidence of the dyke with the strip of gravel has no more significance than that both occupy the bottom of the valley.

Secondly, as to structure. When the dyke was originally cut, the soil from it was, at any rate in part, heaped up on both margins, but has largely been removed by the plough. Towards the Harpenden road, the southern bank is the larger, about seven feet high. Towards the Sandridge road, either the two marginal banks are of equal height or the northern is slightly the larger. It is clear that the ditch, not the banks, was the primary object of the constructors. If the relative size of the banks, however, is to be taken as a criterion at all, it may perhaps be said that, on the whole, the more southerly bank was slightly the more important, *i.e.*, the earthwork may, on this ground, be thought to have faced north rather than south.

There, for the moment, this problem may be left. Attention may now be diverted to another dyke, the handiwork of the Devil, situated to the west of the river Ver and to the north of the successive prehistoric and Roman cities of Verulamium. At the present time, 160 yards of this dyke are clearly visible close to Mayne's farm ; it is fifty feet across —about half the size of the Beech Bottom dyke—and has a low bank on its southern margin. Slight surface-indications hint that it formerly extended eastwards to the water-meadow of the Ver, and a series of nine cuttings in 1932 not only proved its westward extension into Gorhambury park but actually revealed its end in this direction, at the base of the hillside 180 yards west of the lodge gates and 80 yards south of the park-drive (see map, fig. 1). Incidentally a few small sherds of Belgic pottery (forms indeterminate) were found in the filling of the ditch.

Here, then, on both sides of the Ver, are stretches of pre-Roman boundary-dyke, varying in size but similar in construction and, above all, similar in their unusual location. They are both valley-dykes and, so far as can be judged, faced northwards. It is inevitable to associate them with each other : to suppose that the Beech Bottom dyke formerly continued southwestwards down the re-entrant to the river Ver a little above the St. Michael's ford ; that the boundary was continued northwestwards for nearly a mile by the marshy river-valley itself ; and that it was then (at least in its final form—see below) completed

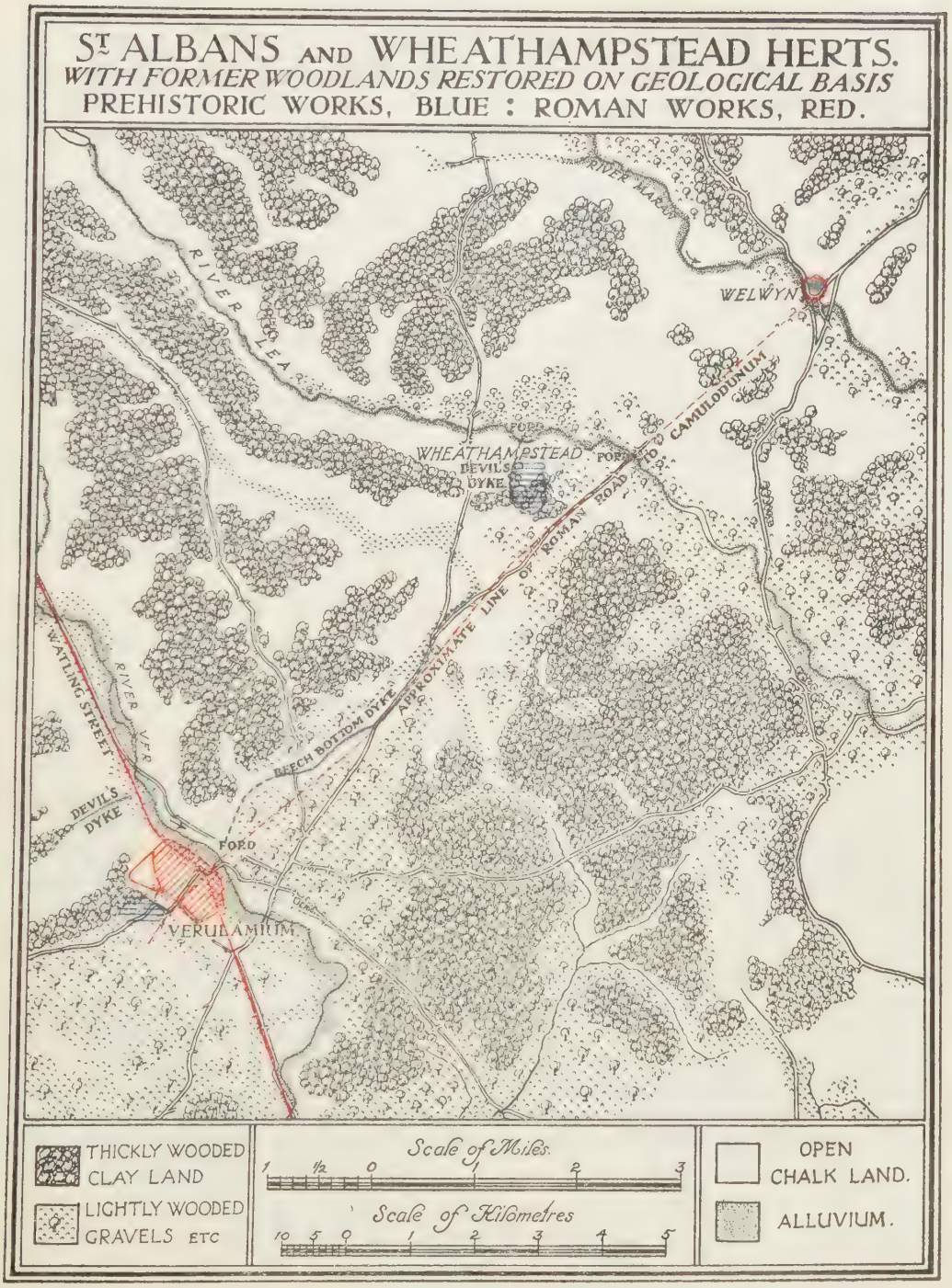


FIG. 2

facing p. 24

westwards from the river to the former forest by the Devil's Dyke (see fig. 2). The whole scheme is in itself an entirely natural one topographically.[3] Considered historically, in relation to the region as a whole, it not only becomes convincing but compels a further series of inferences which demand consideration.

It is at once clear that the frontier-system, so far as we have already traced it, fulfils three closely-related functions. It includes the St. Michael's ford—the only easy crossing of the Ver for a considerable distance in either direction, and the determining factor, therefore, in the location of the prehistoric and Roman cities of Verulamium. Its northern limb, the Devil's Dyke, without unduly ' crowding ' the area of settlement, straddles the approach to pre-Roman Verulamium along that brief stretch of open and level valley which later carried the Roman Watling Street and may already have borne a less disciplined highway. And, not least, the frontier-ditch emphasized, for all to see, an open transverse valley which forms a natural line both of demarcation and of traffic across the countryside.[4] It remains to enquire the reason for the choice of this particular frontier-zone, and it will be seen, I think, that an answer can now be given.

III. The Wheathampstead Oppidum

Five miles northeast of the ford across the Ver at Verulamium, another ford still carries the wayfarer across the rather wider valley of the Lea. This crossing is at Marford, on the eastern outskirts of the large village of Wheathampstead. About a mile downstream, at Waterend, is a second ford, overshadowed by the pleasant Jacobean house where Sarah Jennings, first Duchess of Marlborough, is said to have been born. On the southern brow of the valley, overlooking these fords but nearer to the former, is the rampart of one of the most gigantic earthworks in Great Britain. It is towards this that the Beech Bottom dyke, and the contours which control it, point. To this, also, the whole

[3] It is easy to find parallels for the amalgamation of earthwork and river in an ancient boundary. For example, several notable instances along the line of Offa's Dyke have been noted by Dr Cyril Fox.

[4] Incidentally, the great steep-sided ditch would break the heart of any cattle-raider. It might be possible for a pertinacious cattle-driver to slide a herd of cattle down one side of the Beech Bottom dyke; but, once in the point of the ditch, the animals would scatter laterally, and it would require an almost superhuman pertinacity to drive them up the opposite side.

BELGIC OPPIDUM AT WHEATHAMPSTEAD, HERTS.
FORD
Marford Farm
RIVER LEA
300
300
MARFORD
← To Wheathampstead
Old Pit
To Lemsford →
DEVIL'S DYKE
A
B
C
Dyke Nurseries
THE SLAD
Dyke Lane
Beech Hyde Lane
Lower Beech Hyde Farm
THE MOAT
Samuels Farm
Scale of
100 50 0 100 200 300 400 500 600 700 Yards
100 50 0 100 200 300 400 500 600 700 Metres

Fig. 3

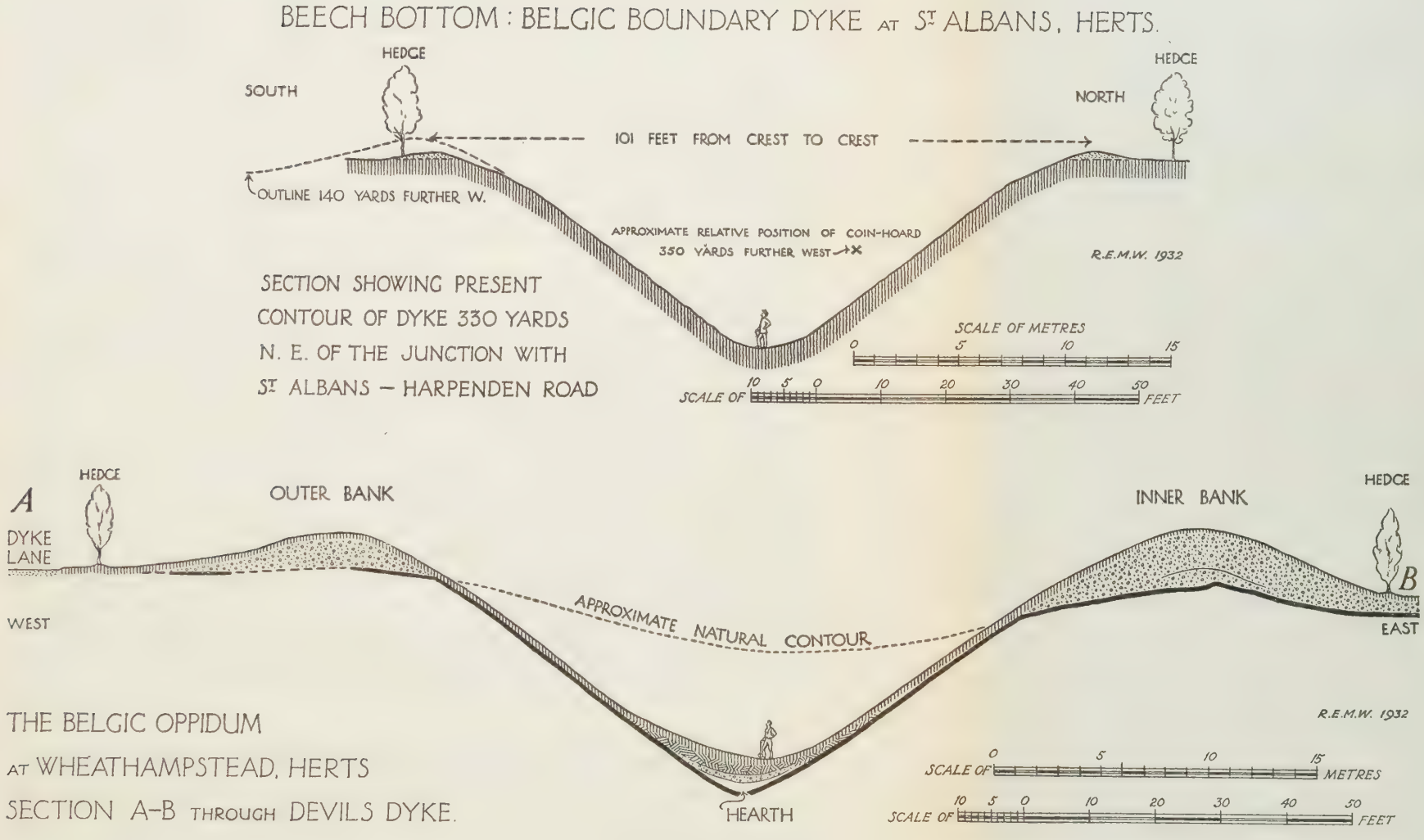


FIG. 4. Compare FIGS 7 and 9

facing p. 26

character of the dyke—its vast scale, its abrupt sides, its somewhat indifferent use of banks—leads the enquirer in search of analogy. When Mr O. G. S. Crawford, with his flair for the potentialities of an archaeological problem, dragged me first to Wheathampstead in 1930, on the eve of the Verulamium excavations, it was clear to both of us that the builders of the Beech Bottom dyke and of the Wheathampstead earthworks were the same people. Now, in 1932, with the fuller knowledge of the frontier-dyke, it followed (if we were right) that Beech Bottom and the Verulamium Devil's Dyke were relics of a transverse *limes* covering the whole tract of interfluvial country which depended, on the one hand, upon the Ver ford and Belgic Verulamium and, on the other hand, upon the Lea ford and the Wheathampstead earthwork. It remained only to insert the word ' Belgic ' before ' Wheathampstead ' to complete the symmetry of the theory. In other words, the partial excavation of Wheathampstead was forced upon us as a complement to the excavation of Belgic Verulamium.

Accordingly, with the ready compliance of Sir Charles Nall-Cain, Bart., and his tenant, the Verulamium Excavation Committee set to, and, in August and September 1932, under the immediate supervision of Mr J. E. Broad, Mr Kenneth Oakley and Miss N. Champion de Crespigny, a series of trial-trenches was cut through the defences and in the interior of the earthwork. The details of the site and its exploration need not detain us here. Suffice it to note that, on plan (fig. 3), the earthwork enclosed an area of at least a hundred acres ; that its western and eastern sides (known respectively as the Devil's Dyke—not to be confused with the Verulamium Devil's Dyke—and the Slad) are, in origin, shallow re-entrants generously enlarged by man to a maximum width of 130 feet and to the astonishing vertical depth of 40 feet, with the earth spread mainly on the inner lip but partly also on the outer lip (figs. 4, 8 and 9); that a portion of its southern end has been completely defaced or is non-existent ; and that its northern limit, unless represented by the Wheathampstead–Lemsford road, is not now visible.

The cutting through the defences (fig. 8) revealed no relics within the banks and no trace of previous occupation underneath them. In the extreme bottom of the ditch, *under* the 'rapid silt', were a hearth and a sherd of wheel-turned Belgic pottery. The area within the defences has in part a gravel subsoil, but is mostly capped by a cold and sticky boulder-clay which must have been cleared of a fairly dense woodland by the original occupiers. No huts were uncovered during

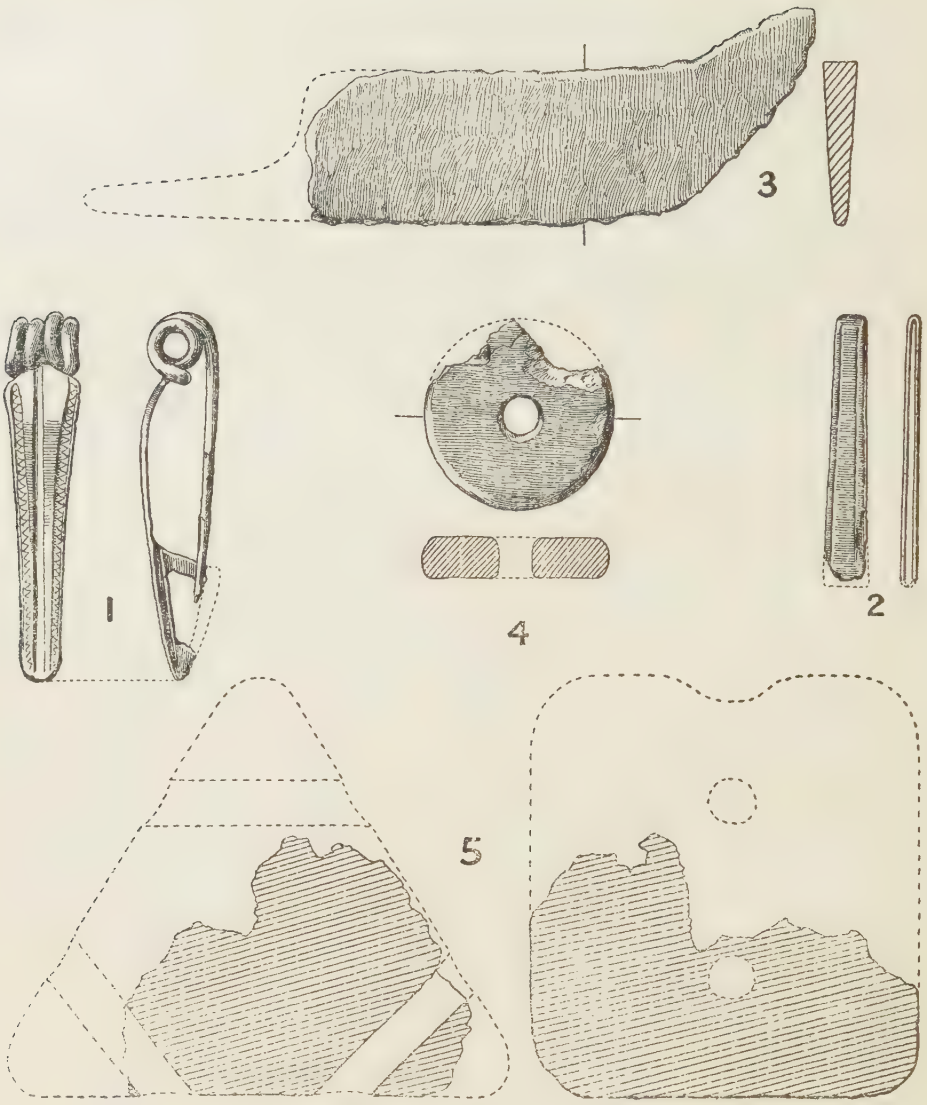


Fig. 5

THE WHEATHAMPSTEAD OPPIDUM:
1, bronze brooch; 2, bronze tweezers; 3, iron knife; 4, clay spindle-whorl; 5, clay loom-weight (sections).
Scales: 1–3, $\frac{1}{1}$; 4 and 5, $\frac{1}{4}$

the recent excavations, but a considerable stretch of two prehistoric U-shaped drainage-trenches, similar to those which are characteristic of Belgic Verulamium and Belgic Colchester, was cleared out at 'C' on plan (figs. 3 and 10). Into these trenches, during an ancient clearance of the adjacent surface, had been thrown innumerable pot-sherds, a few fragments of a triangular loom-weight of normal Early Iron Age type, a clay spindle-whorl, an iron knife, a pair of bronze tweezers and a bronze brooch (fig. 5). The brooch is a variant of the continental 'Nauheim' type (probably of mid-Gaulish origin) which is found on sites of the Mont Beuvray series in the first century B.C. But the large mass of pottery is the main clue to the history of the site. A detailed discussion of it would be inappropriate to the present context, but the salient features may be noted (figs. 11–13):—

(i) It is all wheel-turned, the fabric varying from coarse thick gritty ware to smooth, grey or black ware.

(ii) Much of it is cordoned, and most of it is of, or akin to, recognized Belgic types.

(iii) Only three or perhaps four pedestals, and those very hesitant ones (fig. 11, nos. 1 and 4–6), occur; indeed, the suspicion is growing that, at the time of the Belgic invasion of Britain, the ancient pedestal-urn type was already regarded as archaic, and was rarely used save for ceremonial purposes, notably burials.

(iv) There are no butt-beakers, no imported Italic wares and no imitations of Italic wares such as occur abundantly both at prehistoric Verulamium and at prehistoric Colchester.

This last feature contains the essence of the matter. With the doubtful exception of a few amphorae, the Italic wares—notably Arretine—and provincial equivalents first spread across the northern frontiers of Rome in the reign of Augustus, during the last quarter of the first century B.C. After the middle of the reign of Augustus these wares become normal in Belgic Britain. The pottery from Wheathampstead is abundant; it is devoid of Italic taint; it is therefore substantially pre-Augustan. With this conclusion another factor is consistent. The post Augustan Belgae of southeastern Britain were sure of themselves; boundary-dykes apart, their principal cities—Verulamium, Welwyn, Colchester—were either open or but lightly protected.[5] But the monstrous earthwork of Wheathampstead tells a

[5] Belgic Verulamium was open at first. The defences cover evidences of occupation and are secondary (see ANTIQUITY, June 1932, p. 137), and they are probably incomplete. They may be as late as the Claudian invasion. See below, p. 35.

different story. It is the work of men with wealth, power, arrogance ; but it is the work also of men who were still, for one reason or another, uneasy in their adopted land.

The Wheathampstead earthwork is, then, a large Belgic oppidum dating from the early, pre-Augustan phase of Belgic colonization. Its rulers controlled the upper valley of the Lea, and the whole tract of country between it and the Ver. The northern limit of this tract they demarcated by a frontier-dyke of a size unapproached amongst the frontier-dykes of Britain. It may have been the German strain in their blood that showed forth in this ; for we are reminded of the oft-quoted words of Tacitus, who in the first century A.D. describes the German tribe of the Cherusci as assembling ' in a spot closed in by a river and by forests, within which was a narrow swampy plain. The woods, too, were surrounded by a bottomless morass, only on one side of it the Angrivarii had raised *a broad earthwork as a boundary* between themselves and the Cherusci ' (*Annals*, II, 19). The ' one side ' was evidently the side devoid of natural obstacles such as swamp or forest, just as our Beech Bottom dyke-system is sited across a belt of relatively open country.

IV. Verulamium and Welwyn : offshoots of Wheathampstead

The large admixture of Italic forms, or of copies of Italic forms, amongst all the groups of pottery so far recovered from prehistoric Verulamium (Prae Wood and vicinity) was indicated in the summary published in Antiquity last June. If further excavation shows that these groups are representative—and they are already conclusive for a large area of the site—then we shall be driven to an interesting historical conclusion. We shall be compelled to regard Verulamium as a foundation of the Augustan period (we know that king Tasciovanus struck coins there in the latter part of the first century B.C.), and to discard the conventional identification of it with the city of Cassivellaunus stormed by Caesar in 54 B.C. Caesar, it will be remembered, nowhere names the city. His general indications would fit the Wheathampstead site, only five miles away, just as well as they fit Verulamium. And since now we have at Wheathampstead an oppidum of the Caesarian era, as it seems, and of outstanding magnitude—a place far more worthy of Caesar's powerful opponent than the Verulamium site—it is easier, in the light of the new evidence, to transfer the historic scene from the Ver to the Lea. Verulamium emerges as a later foundation, established or developed by Tasciovanus in the last quarter of the

first century B.C. as an open city (subsequently fortified—see below), at the western end of the Lea-Ver frontier, in a position precisely similar, in its relation to valley and ford, to that occupied by its predecessor at Wheathampstead. Thereafter, if our evidence is representative, Wheathampstead waned as Verulamium waxed; it may be that the partial obliteration of the Wheathampstead earthwork represents a deliberate 'slighting', at this period, of an oppidum already obsolete. Incidentally, on this general view, the 'Devil's Dyke' frontier north of Verulamium and west of the Ver would be an extension, on a lighter scale and in the time of Tasciovanus, of the older Beech Bottom system.

The reason for this transference of focus from the Lea to the Ver valley can only be guessed. Here it will suffice to observe that the Ver valley is the better adapted for an extension of influence in the direction of the midlands; indeed, it later bore the Roman Watling Street. And it is towards the northwest, in the direction of Bedfordshire and Northamptonshire, that the Belgic pedestal-urn culture finds its ultimate limit in the hinterland of Britain. This northward movement may well have occurred during the long reign of Tasciovanus, and, pivoted on Verulamium, may have confirmed the authority and prosperity of the new capital.

In connexion with Wheathampstead and its environs, one other site claims a momentary notice. Three miles northeast of the oppidum the valley of the tiny Mimram or Maran, tributary of the Lea, runs nearly parallel to the major river and is joined from the northeast by the dry valley which now carries the arterial Great North Road. Here, on the south side of the stream, a rich Belgic pedestal-urn cemetery was found and largely destroyed in 1906. The association of numerous Italic imports with the Belgic products implies both considerable wealth and a relatively late date—not earlier, probably, than the end of the first century B.C., and perhaps rather later. The site of the actual settlement has not been identified, but may well have been the low flat-topped promontory at the valley junction, to the north of the stream. Here, then, we are again confronted by a riverside Belgic settlement, apparently later than Wheathampstead and doubtless in some sense derived from it.

In tabular form, the provisional results of the field-work which is the basis of this paper may now be stated as follows:—

1. In the first half of the first century B.C. a major Belgic

oppidum was established beside a ford in the upper Lea valley at Wheathampstead in Hertfordshire. This is more likely than any other known site to have been the headquarters of Cassivellaunus at the time of the Caesarian invasion.

2. The adjacent tract of country, between the Lea valley and a fordable point on the Ver valley, five miles away, was wholly or partly delimited towards the northwest by a formidable valley-dyke (Beech Bottom), which would constitute both a clear political boundary and an effective obstacle to cattle-driving.

3. Later, in the latter part of the first century B.C., began the great period of Belgic prosperity in southeastern Britain, coincident with the conservative continental régimes of Augustus and Tiberius. An open settlement, of some substance, came into being three miles from Wheathampstead on the banks of the Mimram at Welwyn. More important, Verulamium was established beside the ford at the opposite end of the vicinal territory of Wheathampstead, and the frontier-dyke, on a considerably smaller scale, was appropriately extended westwards across the Ver (Devil's Dyke, north of Prae Wood). The ancient fortified city at Wheathampstead, under the changing conditions of a developing countryside, now lost its primary importance, and the new city on the Ver became the royal headquarters. When, a generation later, these were moved once more, to the coastal site of Colchester, Verulamium remained a flourishing Belgic city of secondary political importance. Almost immediately after the Claudian invasion, the prehistoric site was abandoned and the new *municipium* arose on the hillside below it.

V. The Belgic occupation of Britain

The Wheathampstead earthwork, then, appears on the map as a bulwark of Caesarian Britain. As such it may, in conclusion, be considered briefly in its general relationship to the Belgic immigrations.

The Belgae, as they are known to us from many burial-sites and a few settlements, resembled the Germans, from whom they were in part descended, in their predilection for river-valleys. They were not, like the non-Belgic inhabitants of prehistoric Britain, first and foremost a downland folk. Even in non-Belgic Britain, the gravel banks of rivers bore, indeed, a far larger prehistoric population than is sometimes

FIG. 6. OBVERSES AND REVERSES OF PART OF SILVER COIN-HOARD FROM THE BEECH BOTTOM DYKE

1–9 Republican; 10, Galba; 11, Vitellius; 12–18, Vespasian; 19–20, Titus; 21–22, Domitian; 23–24, Trajan; 25, Hadrian $\frac{3}{4}$

facing p. 32

FIG. 7. THE 'BEECH BOTTOM' BOUNDARY-DYKE
Northern slope, with men marking respectively the foot, middle and top. Compare FIG. 4

Fig. 8. THE WHEATHAMPSTEAD OPPIDUM: CUTTING THROUGH THE INNER BANK OF THE 'DEVIL'S DYKE'

FIG. 9. THE WHEATHAMPSTEAD OPPIDUM: INNER SLOPE OF THE DEVIL'S DYKE
The three men respectively mark the foot, middle and top of the slope. Compare FIG. 4

Fig. 10. THE WHEATHAMPSTEAD OPPIDUM: DRAINAGE DITCH AS CLEARED, WITH PARTIALLY EXCAVATED CROSS-DITCH IN FOREGROUND

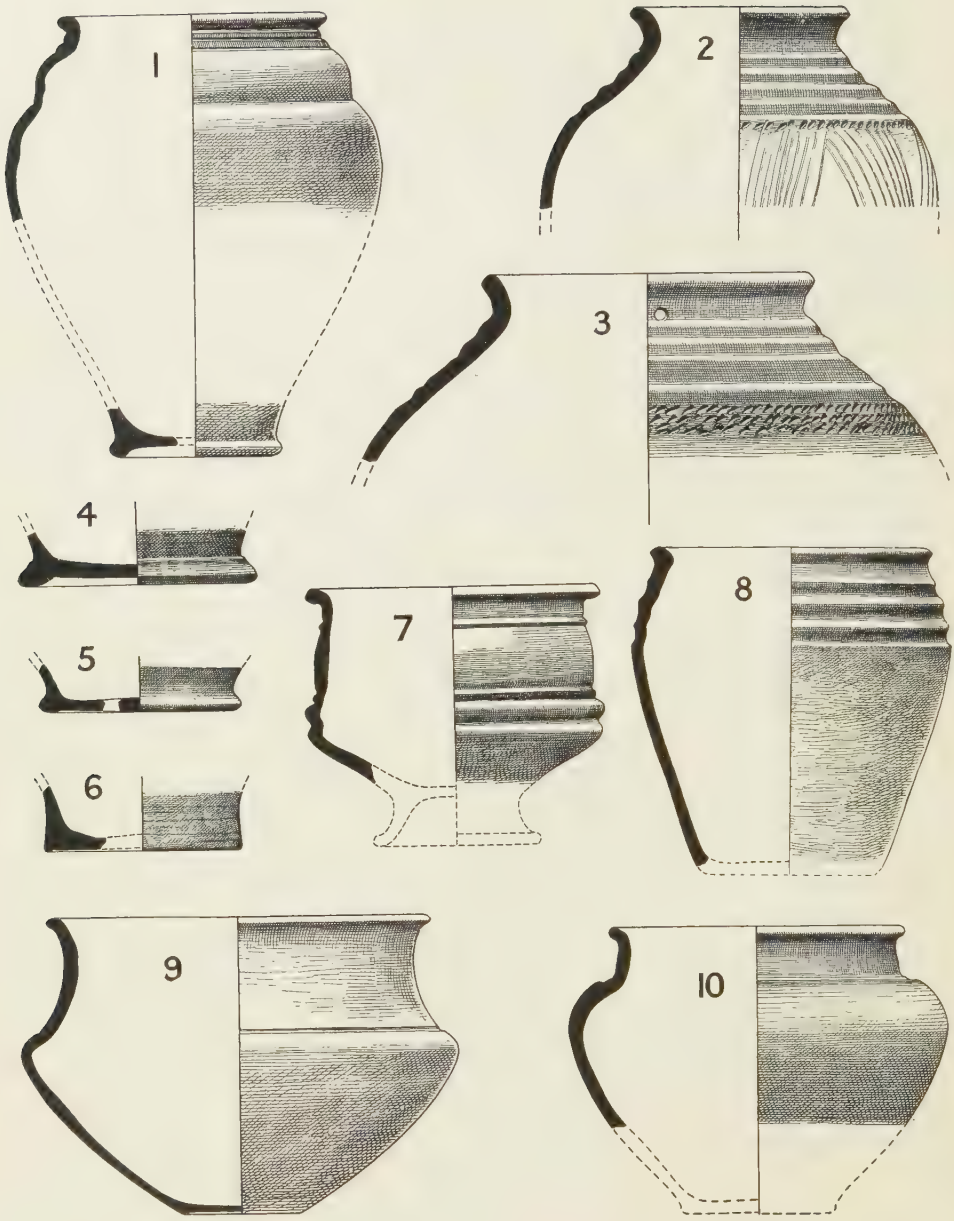


FIG. 11. POTTERY FROM THE WHEATHAMPSTEAD OPPIDUM $\frac{1}{4}$

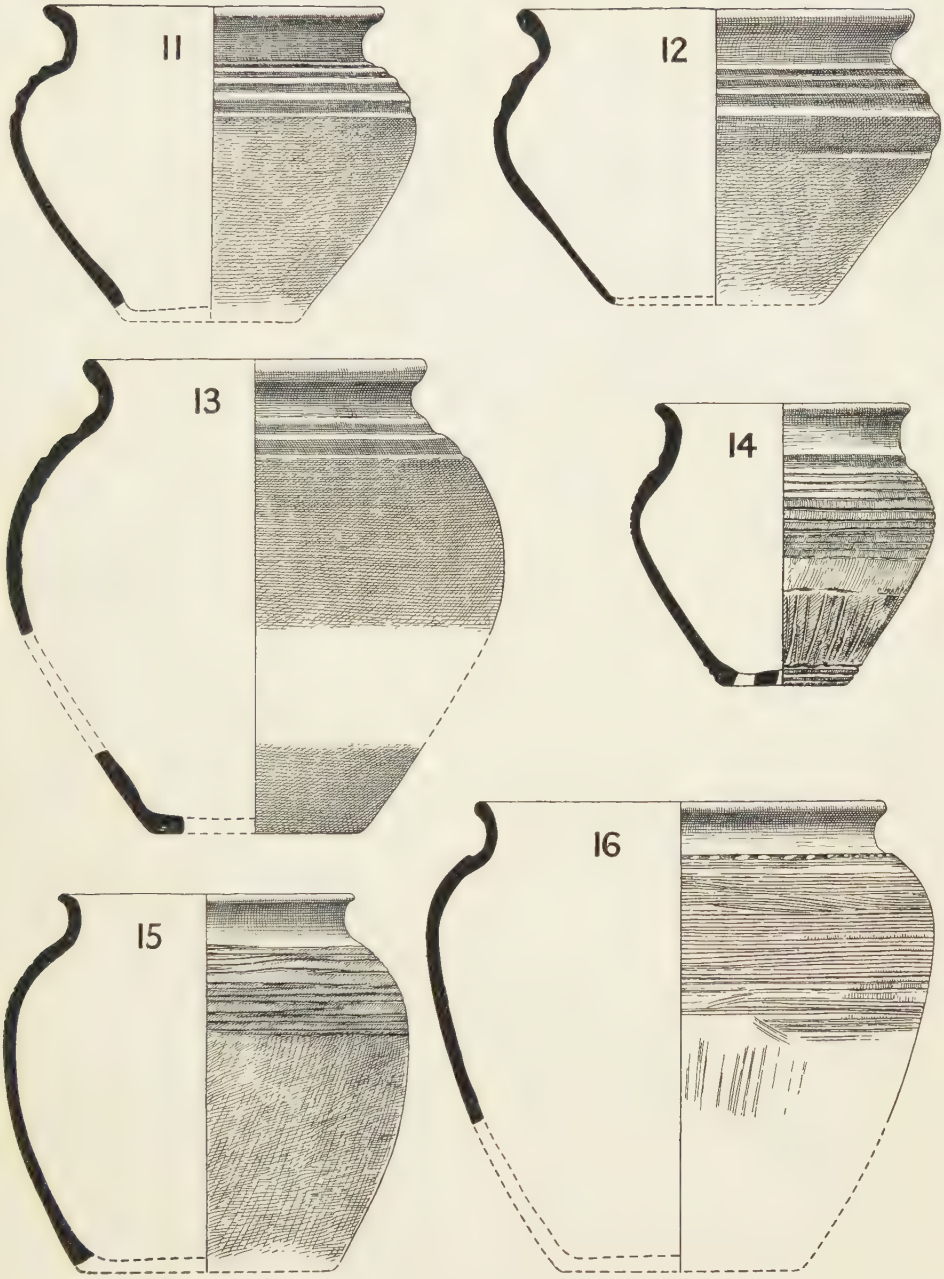


FIG. 12. POTTERY FROM THE WHEATHAMPSTEAD OPPIDUM $\frac{1}{4}$

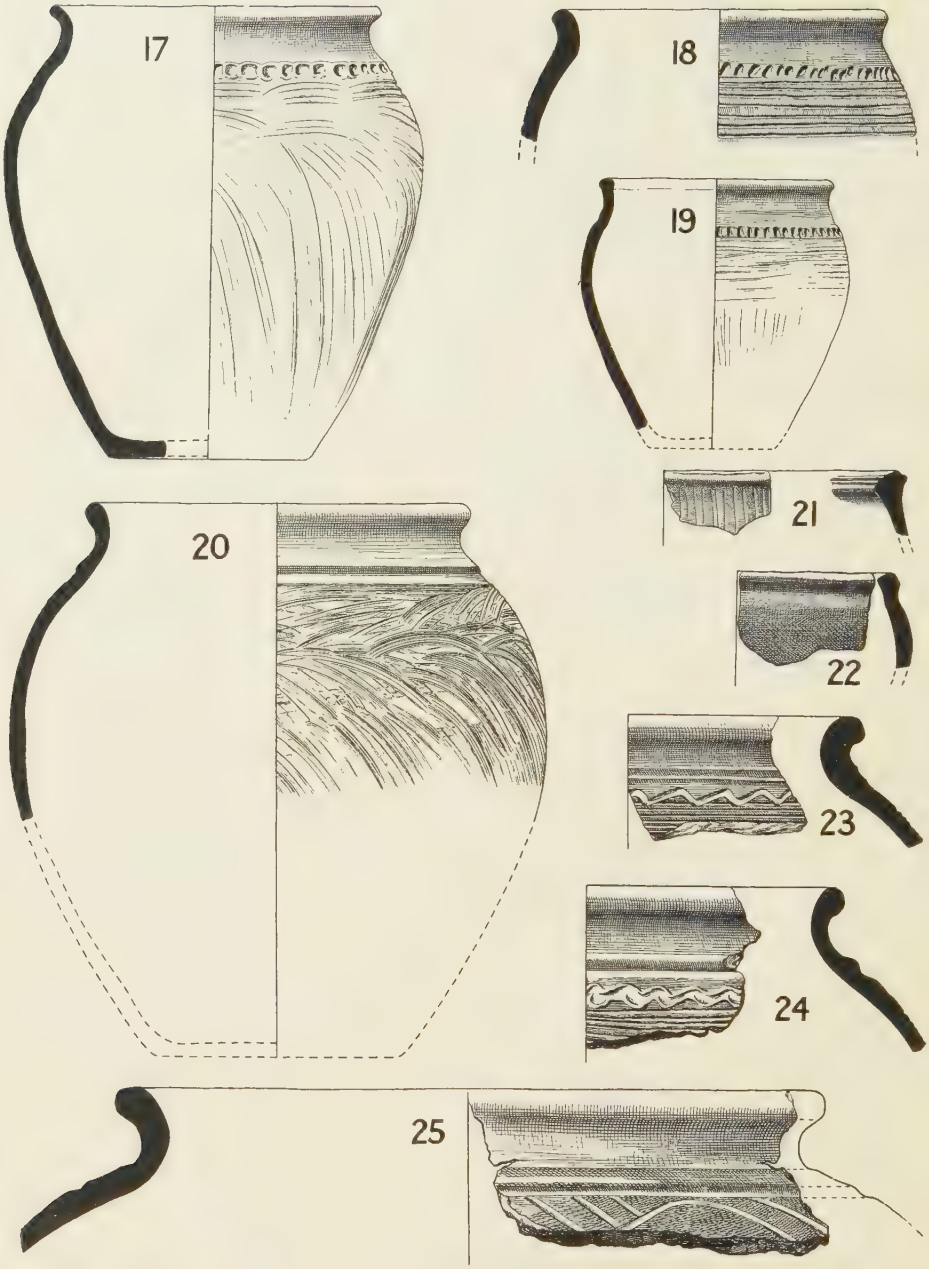


FIG. 13. POTTERY FROM THE WHEATHAMPSTEAD OPPIDUM $\frac{1}{4}$

realized. But the Belgae were more than riverside villagers. They *exploited* the rivers, appropriated the fords, used and controlled the river-system as a part of a scheme of valley and cross-valley highways. This was substantially a new phenomenon in prehistoric Britain. It was the work of a group of organized and wealthy peoples who were something more than the mere outcast continental surplus from which ancient Britain had often enough been compelled to recruit her population. They had come to the island first to plunder, says Caesar, and then had settled there as tillers of the soil. They were in a sense an anticipation of the Teutonic invaders of post-Roman times ; and, although they lacked the solid Roman foundation upon which the later Saxons were able ultimately to build, it is worth observing, in passing, that even today the Belgic cities of Britain are not, all of them, waste cities—that Colchester, Winchester and one or two others can show some title to a direct Belgic ancestry.

As a valley-folk, the earlier Belgic invaders sought out the Kentish rivers, travelled up the Stour to Sturry and Swarling, and perhaps established their first oppidum in Bigberry Wood, on the low hill-top overlooking Canterbury.[6] Further along the coast, the Medway attracted them to Aylesford and Allington. On the opposite shore of the Thames estuary, they landed at Shoeburyness and made their way up the rivers of Essex to Burnham and elsewhere. But the first important tributary which they encountered on this side of the Thames was the Lea, just below the future site of London. Up this tributary turned the vanguard of the ' invasion ', and in or near the higher reaches of the river brought the earlier phase of the movement to a close. Here the Belgic Catuvellauni, not yet perhaps very numerous, paused long enough to consolidate and develop and delimit their new territory. Here, at Wheathampstead, they built their oppidum ; and here, one may guess, were the headquarters of Cassivellaunus at the time of the Caesarian raids. Later, in a pacified countryside, open towns and villages sprang up, situated characteristically in the neighbourhood of fordable rivers. The old Wheathampstead oppidum—perhaps the unidentified Ricon(ium) or Sego(ntium) at which, in addition to Verulamium, Tasciovanus would appear to have struck coins during

---

[6] For the evidence available from this site, see R. F. Jessup in the forthcoming *Archaeological Journal*, LXXXIX (1932). No prehistoric site in southeastern Britain should better repay excavation—as, in another sense, the gravel-diggers have already discovered.

his long reign—was now largely obsolete and its mantle fell upon Verulamium, the new open city founded by Tasciovanus himself. The development of continental trade which accompanied the

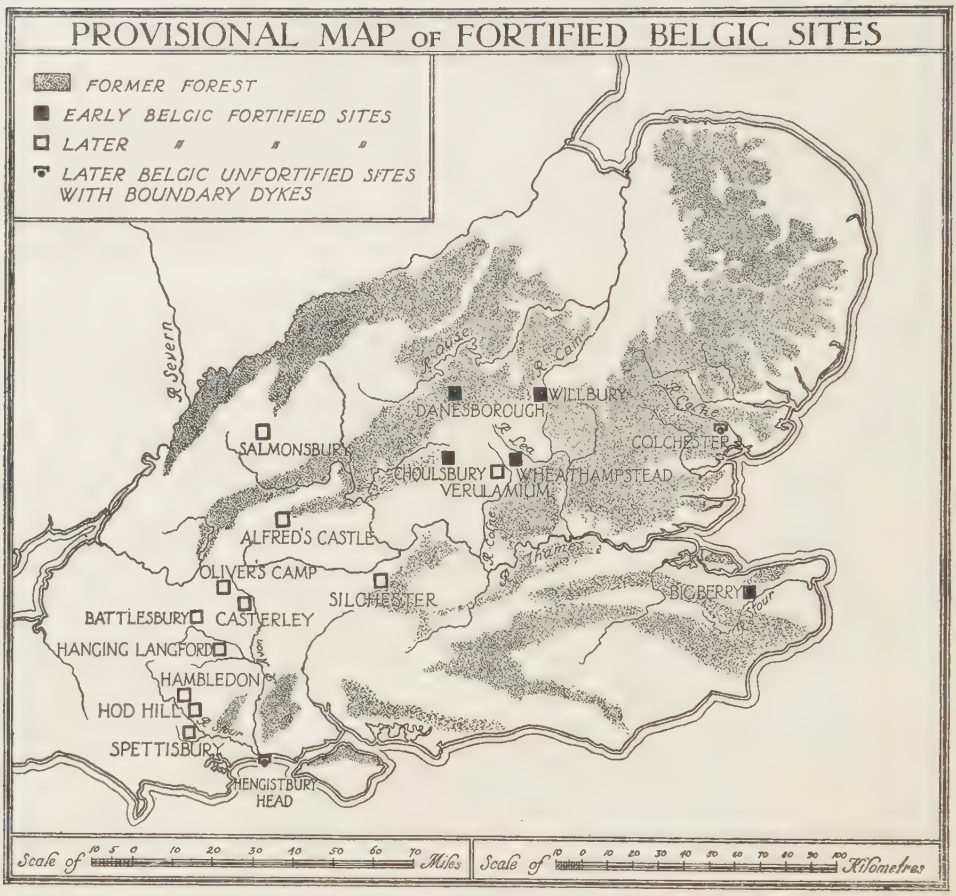


FIG. 14

consolidation of Roman Gaul by Augustus and the consolidation of Belgic Britain by the Cassivellaunian dynasty necessitated, before long, a further transference of the Belgic headquarters to a site nearer to the coast ; hence, at the beginning of the first century A.D., the Colchester

of Cymbeline. Finally came the Roman invasion of A.D. 43. Colchester, the native capital, was the primary objective of the legions. Verulamium must have submitted soon afterwards, but it may have been at this moment that the inhabitants of that city, in a region where the craft of defensive earthwork had been in abeyance for two generations or more, clumsily attempted to fortify themselves with the stockade and the incomplete, rather ramshackle earthwork which can be traced in Prae Wood today. Only in the west, in Wessex and the adjacent regions, did the Belgae of the Bead-Rims—the last phase, it seems, of pre-Roman Belgic immigration—still, at this late period, dwell here and there in fenced cities along the margin of Belgic Britain (fig. 14).[7]

[7] See also map by C. F. C. Hawkes, ANTIQUITY, March 1931, p. 91. In adapting Mr Hawkes's useful map, I have here omitted from it all the 'hill-forts' of the Cambridge region, since a fresh examination of the evidence from those sites, kindly communicated to me by Dr Cyril Fox, makes it very doubtful whether any of them should be ascribed in any significant sense to the Belgae. In other cases it is uncertain whether the actual fortifications are the work of the Belgae, though Belgic occupation is indicated in every instance. Exact classification is often difficult; Hengistbury Head, for example, might fall into any of the three categories. Chichester should possibly be included under the last symbol, but there is no positive evidence.

British Maglemose Harpoon Sites

by H. and M. E. Godwin

School of Botany, Cambridge

DURING the improvement of the climate of Europe which followed the retreat of the ice at the close of the last glacial period the continent suffered re-invasion by successive waves of immigrating plants at rates determined partly by their natural climatic range and partly by their relative capacity to spread by seeds or by vegetative means. Of this migratory flora the trees are conspicuously important, firstly in that they dominated by their life-form the other components of the vegetation, and secondly in that, being wind-pollinated, they produced vast quantities of pollen which were carried by the wind over wide distances and incorporated in any deposits then forming. In peat and in estuarine and coastal silts particularly, conditions largely inhibited bacterial decay of the spores, the outer membranes of which remain recognizable in microscopic examination. Systematic analysis by the methods introduced by Von Post has now been applied over a large part of Europe and very considerable resemblance is evident in the migratory sequence of forest-types over the whole continent. Since the post-glacial climatic optimum, the climate of the continent has suffered further fluctuations and these are also characterized by regular and well marked changes in the forest-cover of the land as indicated by the fossil pollen-content.

It therefore follows that, when due allowances have been made for latitude and longitude of a given site, for its relation to the continental margin and so forth, the sequence of contained fossil pollen will afford a general post-glacial time-scale which can be, and in fact has been, correlated with such indices of post-glacial chronology as the beaches of the Baltic seas, the glacial lake-deposits of De Geer, and the broad divisions of archaeology. As an example we may refer to the left-hand section of fig. 1, which shows the complete sequence of post-glacial pollen deposition in central and north Esthonia as worked out by Thomson (1). A very well-marked series of forest-types is shown, with a clear general correlation with the Blytt and Sernander climatic periods and other dating points. Similar diagrams have been constructed for many parts of Europe, and it is clear that they can afford great assistance in solving problems of post-glacial stratigraphy.

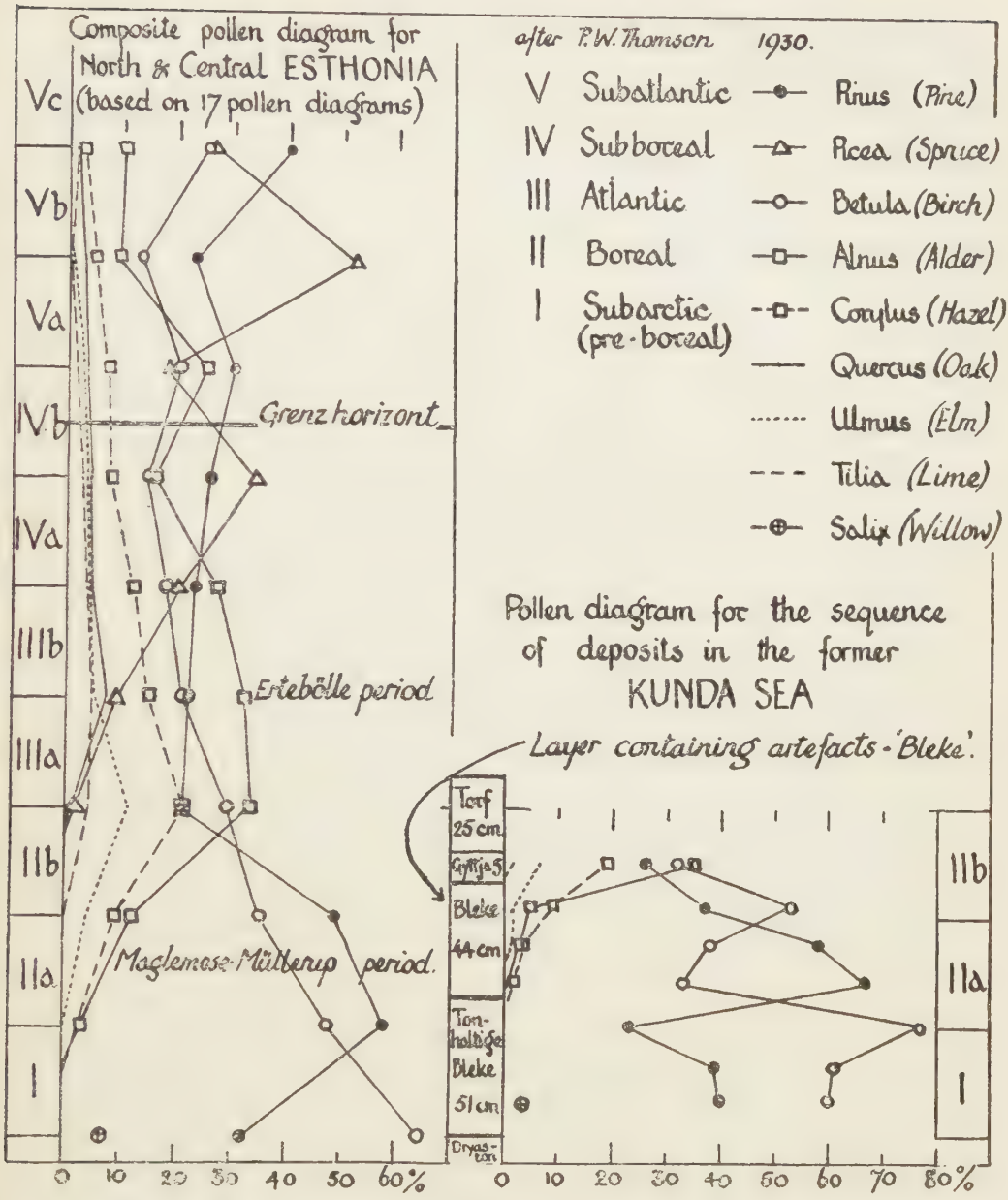

Composite pollen diagram for North & Central ESTHONIA (based on 17 pollen diagrams)
after P. W. Thomson 1930.
V Subatlantic
IV Subboreal
III Atlantic
II Boreal
I Subarctic (pre-boreal)
Pinus (Pine)
Picea (Spruce)
Betula (Birch)
Alnus (Alder)
Corylus (Hazel)
Quercus (Oak)
Ulmus (Elm)
Tilia (Lime)
Salix (Willow)
Vc
Vb
Va
IVb
IVa
IIIb
IIIa
IIb
IIa
I
Grenz horizont
Ertebölle period
Maglemose-Müllerup period
0 10 20 30 40 50 60%
Pollen diagram for the sequence of deposits in the former KUNDA SEA
Layer containing artefacts - 'Bleke'.
Torf 25 cm
Gyttja 5
Bleke 44 cm
Ton-haltige Bleke 51 cm
Dryas-ton
0 10 20 30 40 50 60 70 80%

FIG. 1

ANTIQUITY

The problem to which the method is applied in this paper is the determination of the age of three bone harpoons discovered in England since 1903. Two specimens come from East Yorkshire, where they were discovered by Mr Morfitt at Skipsea and Hornsea in the years 1903 and 1905 respectively. They were not described until 1922 when Mr A. L. Armstrong claimed that they were of Maglemose culture (4). This claim evoked severe controversy, and the two important committees formed to consider the authenticity of the harpoons expressed their opinion, based largely on the evidence of the implements themselves, that the harpoons were genuine Maglemose artifacts. They resemble however the more developed type of those fabricated in Kunda, Esthonia rather than the type found in the original Maglemose station at Müllerup, Zealand. It happens that close pollen statistical analysis has been made both of the Esthonian and Danish sites and in the present paper the authors describe similar analysis of the peat deposits in which the Yorkshire harpoons were found. By this means it has become possible to correlate the Yorkshire beds at Skipsea quite closely with those of Kunda and Esthonia, the results supporting in the strongest possible manner the original claim that the harpoon found there was of Maglemose age.

In the summer of 1932 the skipper of the steam drifter *Colinda* procured another bone harpoon of Maglemose type in a sample of 'moorlog' (peat) brought up by the trawl from between the Leman and the Ower banks off the Norfolk coast. The site is about 25 miles (40 km.) from the coast, and the 'moorlog' lies in about 19–20 fathoms (6 m.) of water. Unfortunately the original 'moorlog' was not kept, but further samples trawled from as nearly as possible the same site have been subjected to pollen-analysis by the authors, again with a view to establishing more closely the age of the harpoon. The pollen-content again shows close relationship to the Maglemose culture-layers of Zealand and Esthonia.

Skipsea

The Maglemose harpoon discovered by Mr Morfitt in 1903 occurred in the bed of an old mere exposed during coastal erosion just north of Atwick on the Yorkshire coast. The implement was found in the silt below five feet of peat during the excavation of the skeleton of an elk (*Cervus giganteus*). Remains of reindeer were found on the harpoon level and the silt was later recognized as containing a fresh water Ancylus fauna. Following the controversy which arose as to the

authenticity of the Holderness harpoons Armstrong investigated the mere site, and from the silt underlying the peat and from the basal peat was able to describe a series of flint implements of Maglemose type.

In the summer of 1932 the authors were enabled by the courtesy of Mr Armstrong to see the mere site in his company and to take a complete vertical series of samples for analysis through the mere deposits. Continued coastal erosion had by this time left the harpoon site some yards seaward but a good section of the mere was still evident as a shallow peat bed crescentic in section about 100 yards wide, about 9 feet thick in the middle, and tapering out to nothing at the margins. At the sampling site the section was as follows :

(0·76 m.) 2 feet 6 inches, fine brown clay—now cracking into columnar form. A fresh water deposit.

(2·13 m.) 7 feet, solid black or brown amorphous peat with large numbers of horizontal tree branches or trunks, including much oak (*Quercus*) especially in the upper two feet, which is almost solid with them. Hazel nuts found at 4 feet 4 inches, 5 feet 6 inches and 6 feet 4 inches, from the top of the bed, and at its very base.

(0·15 m.) 6 inches, brown sandy silt—with fragments of *Pinus* bark, fins of pike (*Esox lucius*) and flint artifacts. Stone fruit of (?) *Prunus*. Buttery Blue Clay.

During the sampling further artifacts were found in the sandy silt just underlying the peat : these are in the possession of Mr Armstrong and clearly are of the same type as those described by him in 1923.

The first analysis was of 20 samples taken through the full extent of the mere deposits—at this place 7 feet 5 inches (2.26 m.) thick. The results are given as (a) series in fig. 2, and they show especially in the basal samples extremely striking composition and change in pollen-content. The salient features of the lower samples are these :—(a) the immigration of *Alnus* (alder) is shown by its total absence from the lowest sample and its rapid rise to dominance in succeeding samples in which it reaches respectively 1, 18 and 50 per cent. of the total tree-pollen ; (b) progressive diminution in the percentages of *Pinus* (pine) and *Betula* (birch) pollen ; (c) an extremely high initial *Corylus* (hazel) percentage (over 200 p.c. of the total tree-pollen—in which hazel itself is not reckoned), falling very rapidly to lower values (about 30 p.c.) though with a pronounced secondary maximum ; (d) a small percentage of *Salix* (willow) in the basal sample dwindling to nothing in samples above ; (e) *Tilia* (lime) is present in very small amounts in the samples below 6 feet, and is absent altogether from the two lowest ;

(f) *Ulmus* (elm) and *Quercus* (oak) show high percentage values in the lower samples.

The two tree genera characteristic of the *early* Boreal period all over northwest Europe are *Pinus* and *Betula*, with the two genera *Salix* and *Corylus*, the pollen of which is reckoned separately. High *Corylus* frequencies are extremely characteristic of the Boreal period especially in Europe (8 and 9). It was in late Boreal times that *Quercus* and *Ulmus* immigrated and began to form a substantial part of the total tree-pollen. The immigration of *Alnus* and its rapid increase in frequency, with corresponding diminution in *Pinus* and *Betula* is generally regarded as marking the transition period from late Boreal to early Atlantic, and *Tilia* is supposed to have immigrated at about this time. It will be seen that by all these criteria the basal samples from the Skipsea (a) series (*e.g.* 7 feet to 7 feet 5 inches) occur at the transition from the late Boreal to the early Atlantic period.

In order to test more thoroughly the validity of the basal analyses a second series of samples was collected from the same site but nine samples were taken in the bottom 6·5 inches (16 cms.), this distance covering the zone in which most of the artifacts have been found. The results of the analyses are shown in series (b), fig. 2, and it will be seen at once how strongly they support the results of series (a). In all cases closely comparable percentages of pollen were obtained save that the lowest sample of (b) appears to be a little older than the lowest of (a), and correspondingly shows higher *Ulmus* and *Salix* (33 p.c.) and lower *Quercus*. These facts however only strengthen the case for the validity of series (a), and it is especially noteworthy that there is no *Alnus* pollen at all in samples 1 and 2 of series (b).

It remains to apply these results to the actual harpoon site itself and the possibility of this depends on the contemporaneity of the basal silt right across the mere. A further analysis (no. 10) bearing on this point is that of a sample taken from the very base of the silt much nearer the mere margin than the site we have been describing. The peat here was only 3 feet (1 m.) in thickness, and it seems unlikely that the harpoon site could be younger than this since (a) it was covered by a greater peat thickness and (b) it was exposed on the beach itself, *i.e.* some feet lower in level than no. 10, and therefore would be presumably in a deeper part of the mere and one in which deposition would tend to occur earlier. The sample on analysis yielded percentages for *Pinus* 5, *Betula* 11, *Quercus* 21, *Ulmus* 15, *Tilia* 1, *Alnus* 47, *Corylus* 51. This composition can only be matched satisfactorily in the samples of

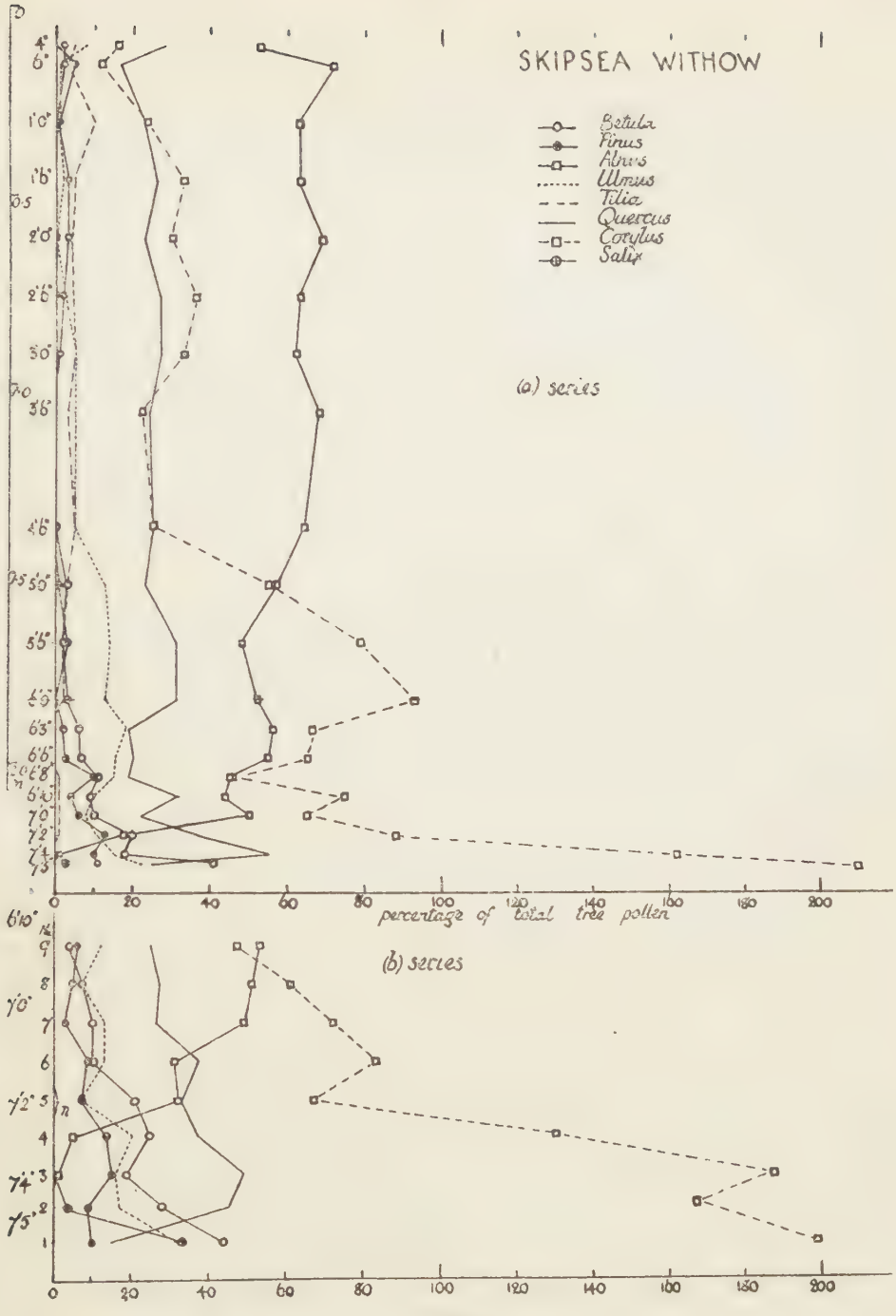

SKIPSEA WITHOW
Betula
Pinus
Alnus
Ulmus
Tilia
Quercus
Corylus
Salix
(a) series
(b) series
percentage of total tree pollen

FIG. 2

levels about 6 feet 10 inches or 7 feet of the (a) and (b) series. Since these are about the period of transition from Boreal to Atlantic, the harpoon site is almost certainly rather older than this, *i.e.* late Boreal.

This of course accords completely with the presence of the remains of elk, reindeer and the Ancylus fresh water fauna of the silt, all of which probably disappeared in this country before Atlantic times.

There is little point for the purpose of the present paper in discussing the upper samples of the Skipsea (a) series but it may be noted that even the upper samples do not show anything of an age younger than Atlantic. The relatively high *Tilia* values in the upper samples may at a later date help towards correlation with other coastal deposits and a closer estimate of age (10).

The Leman and the Ower Banks

From the ' moorlog ' sample trawled from the site of the harpoon discovery two analyses were made, one close to the top and the other near the base, which was recognizable as having been the line of breakage from the underlying sandy rock.

Peat sent from this site to Dr Erdtman of Stockholm is said also to have come from the same sample, and we have put all four into what appears to be their natural sequence (11).

	Betula (Birch)	*Pinus* (Pine)	*Alnus* (Alder)	*Ulmus* (Elm)	*Tilia* (Lime)	*Quercus* (Oak)	*Corylus* (Hazel)	*Salix* (Willow)
H and M.E.G. (top) ..	16	64	—	—	—	17	80	—
Erdtman	12	74	—	2	—	12	47	—
H and M.E.G. (bottom)..	13	85	—	—	—	1·4	81	1
Erdtman	43	57	—	—	—	0	39	—

The agreement between our results and those of Erdtman is quite satisfactory and all indicate the early Boreal age of the samples. The absence of *Alnus* from all four samples, the virtual absence of *Ulmus*, and the absence of *Quercus* from the basal sample, suggest that this peat may be rather older than any examined from the Skipsea site.

Later in 1932 the authors obtained by the courtesy of Dr Muir Evans new ' moorlog ' samples trawled by the *Colinda* on a return visit to the Leman and the Ower banks. This came from between the two north buoys of the Leman and the Ower.

The analysis of specimens from the top and bottom of the sample (4.5 inches [11 cms.] apart) shows the following composition.

	Betula	*Pinus*	*Alnus*	*Ulmus*	*Tilia*	*Quercus*	*Corylus*	*Salix*
a ..	30	69	1	—	—	—	4	2
b ..	75	23	—	—	—	—	7	—

The harpoon was stated to have been discovered on breaking open a piece of 'moorlog' so that if the above samples represent the full depth of the 'moorlog' on the banks the harpoon must be of early Boreal age. This conclusion is however subject to the confirmation of the fact that there is no 'moorlog' on the Leman and the Ower banks of a different age from those samples which have been examined here.

Hornsea

It is recorded by Mr Armstrong (4 and 5), quoting from the records of Mr Morfitt, that the Hornsea harpoon was discovered during the excavation for a gas-holder, 12 feet from the ground surface and in a peat bed containing the remains of reeds, etc. The evidence of subsequent excavation and boring on the gas works site, though it supports the idea that a peat bed is present about 12 feet (3.65 m.) from the surface suggests that the bed is very thin and probably only developed locally.

The attempts of the present authors to secure samples for analysis from this bed have so far been unsuccessful. The borings they have had made apparently pass at 12 feet from the present surface through a mixture of fine gravel and clay containing tiny black fragments probably of organic matter.

Esthonia

The archaeological finds at Kunda come from a layer of 'bleke', a fresh water calcareous deposit about 1 m. in thickness, which was laid down in the shallow basin of the Kunda Sea. This deposit overlies a clay containing remains of sub-arctic plants and with a corresponding content of *Betula*, *Pinus* and *Salix*. The calcareous deposit is overlaid by a widespread sandy layer formed by a catastrophic invasion of the Kunda Sea by the Baltic, and indicating the termination of the lake period in which the Kunda artifacts could have been laid down. Not

only has careful pollen analysis been made of the calcareous lake deposit itself, but also of the beds above and below it so that the most exact limits can be set to the age of the artifact deposits. The pollen analysis of the Kunda Sea deposits has been done by P. W. Thomson and he gives his results in a diagram from which fig. 1 has been redrawn (1, 2 and 3). On the left of the diagram is the generalized pollen sequence from north and central Esthonia and this has been closely correlated with the beaches made at the maximal transgressions of the Ancylus and Litorina seas. The Ancylus maximum is in the middle of the Boreal period of Esthonia, corresponding exactly with a maximum in *Pinus* pollen. The Litorina maximum occurs in the middle of the Atlantic period and corresponds with the maximal extension of the mixed oakwoods and the post-glacial climatic optimum of Von Post. Here, as throughout the post-glacial sequence, the pollen analyses indicate close correspondence between Esthonia and south Sweden.

The pollen analyses for the Kunda Sea deposits are given on the right of the same figure and show, in comparison with the generalized Esthonian diagram, a very clear indication of the Boreal age of the layer containing the artifacts. The essential features are those characterizing the Boreal period over all northwest Europe, *i.e.*, the *Pinus* maximum and subsequent fall, the diminishing *Betula*, the immigration and rapid increase of *Alnus* and the immigration of *Ulmus*. *Quercus* is not yet present, and *Corylus* is present only in small amount, both features which tend to characterize the Boreal period in the more northerly and continental parts of Europe (see Von Post, 9, and Erdtman, 8).

Other artifacts of the Kunda type have also been found in Esthonia in the Embachtal near Dorpat, and Thomson's analyses for this site show the same age as those for Kunda. The analysis shown in the following is for the total thickness of the lake deposit in which the implements were found.

EMBACHTAL

	Betula (Birch)	*Pinus* (Pine)	*Alnus* (Alder)	*Ulmus* (Elm)	*Tilia* (Lime)	*Quercus* (Oak)	*Corylus* (Hazel)	*Picea* (Spruce)	*Salix* (Willow)
'Kalk Gyttja und Bleke'	40	53	—	7	—	—	8	—	—
	51	42	—	7	—	—	3	—	—
	71	28	—	1	—	—	1	—	—

It will be seen that the lake deposit is entirely of Boreal age.

POLLEN ANALYSES FROM THE CULTURE LEVELS OF MAGLEMOSE SITES IN ZEALAND, DENMARK. (After JESSEN)

	SITE	LEVEL	*Betula* (Birch)	*Pinus* (Pine)	*Alnus* (Alder)	Mixed Oak Forest (Oak and Elm and Lime)	*Corylus* (Hazel)	*Picea* (Spruce)	*Salix* (Willow)
1	Svaerdborg	Culture level	31	44	14	11	22	—	1
2		Lower limit of ,, ,,	56	39	1	3	9	—	1
3		Upper limit of Culture level	32	52	3	12	55	—	1
4	Müllerup	Lower ,, ,, ,,	39	50	3	7	47	—	1
5		7 cms. below ,, ,,	60	39	—	—	7	—	1
6		5 cms. above Culture level	24	29	28	19	43	—	+
7	Holmegaard	Upper limit of ,, ,,	16	37	30	15	25	1	1
8	W.-Station	Middle of ,, ,,	15	48	21	16	41	—	—
9		Lower limit of ,, ,,	11	78	3	7	18	—	1
10		20 cms. below ,, ,,	76	24	—	—	2	—	+
11		20 cms. above Culture level	16	20	36	28	27	—	—
12	Holmegaard	Upper limit of ,, ,,	4	34	45	17	10	—	—
13	E.-Station	Middle of ,, ,,	7	52	26	15	31	—	—
14		15 cms. below ,, ,,	51	47	—	1	70	—	1

Denmark

In his investigations of the Danish type-stations of the Maglemose culture, Broholm fortunately collaborated with K. Jessen of the Danish Geological Service, who had been one of the earliest workers in Europe to use the method of pollen analysis and who here applied it to all the archaeological stations investigated. His results (12) are given in tabular form on page 45.

The culture-layers in all cases appear to be definitely of Boreal age, coinciding with the immigration of *Alnus*, and showing diminution of initially high values of *Betula* and *Pinus* whilst the mixed oakwoods components are immigrating and increasing in abundance.

Discussion and Conclusions

It has been shown that the sites of Kunda and the Embachtal in Esthonia, of Svaerdborg, Holmegaard and Müllerup in Denmark, of Skipsea in Yorkshire and of the Leman and the Ower banks off the Norfolk coast of England, have pollen spectra practically identical with one another. From this it follows that in so far as the spread of forest trees into Europe during this phase of post-glacial history was synchronous for Esthonia, Denmark and England, then so far must the cultures of these sites be contemporaneous.

There are two lines of argument which can be brought to bear on this point. In the first place the pollen sequences of Kunda and of Denmark are not merely divided into the Blytt and Sernander climatic periods on a basis of comparison of succeeding forest-phases, but both have been correlated with the common geological standards afforded by the changing levels of the Baltic seas. Thus the Esthonian sequence has been related to the maximal transgressions of the Ancylus and Litorina seas, and the Danish sequences are related to the same data by their close resemblance to the south Swedish sequences, which have themselves been directly correlated with the beaches of Ancylus and Litorina times. The validity of the dating attributed to the Danish sequences in this way is confirmed by analysis of the peat submerged in the Sund during the expansion of the Litorina Sea.

In the second place, it has been suggested on archaeological evidence that the Danish sites are older than those of Kunda and of England. If this is so and the age difference is substantial, the similarity of the pollen spectra in the three countries can only be explained by assuming that the forests reached these parts of Europe in the same order as the

cultures, viz. Denmark first and afterwards Esthonia and England together. The geographical situation of the three countries renders this improbable, for Denmark is intermediate in having a climate less oceanic than England and more oceanic than Esthonia, and in lying south of Esthonia and rather north at least of the Leman and the Ower banks off the English coast. These two lines of argument together suggest that forest development was synchronous in Esthonia, Denmark and England and that therefore the cultures in question were approximately contemporaneous.

A very rough estimate based on an application of De Geer's geochronology* to pollen analytical sequences suggests 500 years as perhaps the maximum range of time allowable within this ' approximate contemporaneity '. It remains for archaeologists to say whether such a period is adequate for the development from Maglemose to Kunda cultures.

These results in any case indicate that the sites in Esthonia, Denmark and England are all Boreal in age (Ancylus period) and that they are much more closely related in age to one another than any of them are to sites of the Ertebolle cultures which are in the Atlantic period and correspond roughly to the Litorina maximum.

Part of the expenses incurred in the work have been met from a grant made by the Royal Society of London in aid of pollen-analytic investigations of lowland peats.

REFERENCES

1. P. W. Thomson, Die regionale Entwicklungsgeschichte der Wälder Estlands. Acta et Comm. Univ. Tartuensis, AXVII 2 Dorpat, 1929.
2. —— Das Geologische Alter der Kunda- und Pernaufunde (vorläufige mitteilung) Beiträge zur Kunde Estlands. Bd. XIV, H 1, 1928.
3. —— Geologische datierung Archäologishe Funde in Estland. Fornvannern, 1930.
4. A. L. Armstrong, Two East Yorkshire Bone Harpoons. *Man*, 75, 1922 (see also *Man*, 31 and 83, 1923).
5. —— The Maglemose remains at Holderness and their Baltic counterparts. *Procs. Preh. Soc. E. Anglia*, IV, 1923.

* See ANTIQUITY, II (1928), 308–18.—EDITOR.

REFERENCES, *continued*

6. M. C. Burkitt, A Maglemose Harpoon dredged up recently from the North Sea. *Man*, 138, 1932.
7. H. Muir Evans, *Procs. Preh. Soc. E. Anglia*, 1933.
8. G. Erdtman, The Boreal Hazel forests and the theory of pollen statistics. *Journal of Ecology*, 19, 1931.
9. L. Von Post, Die Postarktischegeschichte der Europäeischen Wälder nach den vorliegenden Pollendiagrammen. Stockholms Högskolas Geologiska Institut, 19, 1929.
10. H. and M. E. Godwin, Pollen analyses of Fenland peats at St. Germans, near King's Lynn. *Geol. Mag.* 1933.
11. G. Erdtman, International Prehistoric Congress, London, 1932.
12. K. Jessen, Conditions géologiques des deux stations de plus ancien âge de la pierre dans la tourbière de Holmegaard. Extrait des *Mém. de la Soc. Roy. des Antiq. du Nord*, 1926–27.

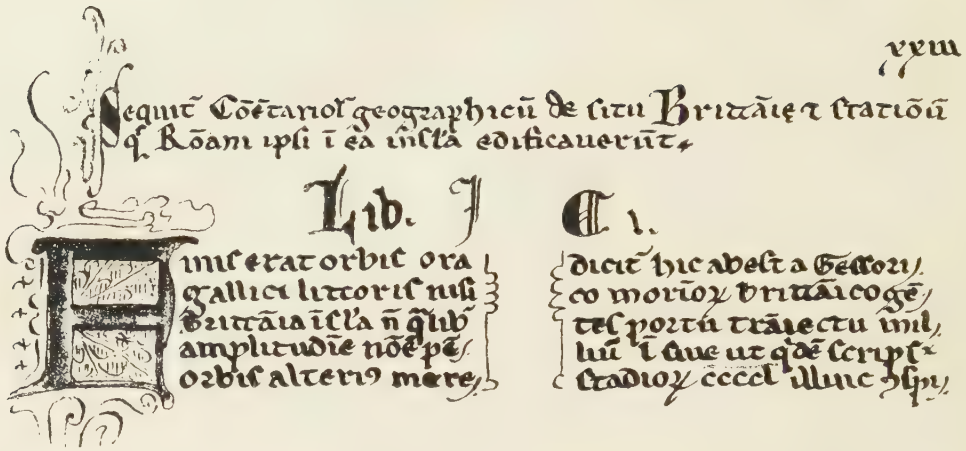

xxm
Sequit Cōētariol geographicū de situ Brittāie t stratiōū
q Rōam ipli i ea insla edificauerūt.
Lib. I Cl.
Imle erat orbis ora
Gallici littoris nisi
Brittania illa ñ glib
amplitudie nōe pe
orbis alteri mere
dicte hic abelt a Gellori
co moriox brittāicoge
tel portu trajectu mil
lū l sue ut qde scripse
stadioy cccel illuc glpi

Fig. 1. THE SPECIMEN OF THE SUPPOSED ORIGINAL MANUSCRIPT OF RICHARD OF CIRENCESTER'S 'DE SITU' SENT BY CHARLES BERTRAM TO WILLIAM STUKELEY

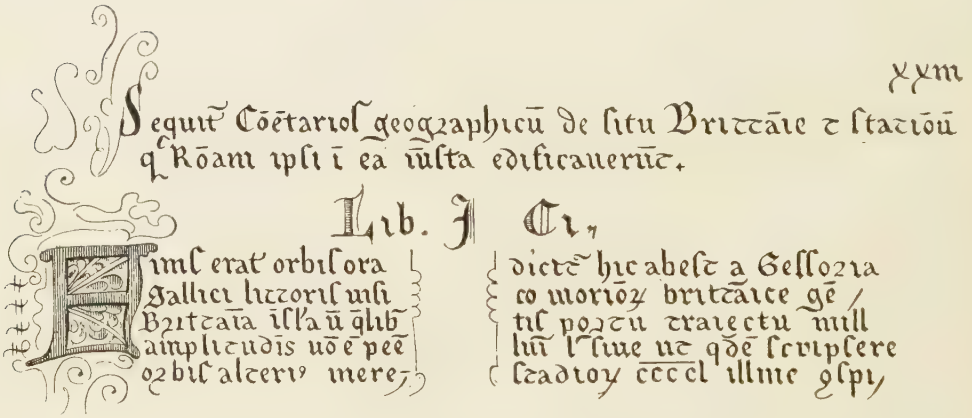

xxm
Sequit Cōētariol geographicū de situ Brittāie t stratiōū
q Rōam ipli i ea insla edificauerūt.
Lib. I Cl.
Imle erat orbis ora
Gallici littoris nisi
Britania illa ñ glib
amplitudis nōe pe
orbis alteri mere
dicte hic abelt a Gellonia
co moriox britāice ge
tel portu trajectu mill
lū l sue ut qde scripsere
stadioy cccel illuc glpi

Fig. 2. FROM WILLIAM STUKELEY'S 'ACCOUNT OF RICHARD OF CIRENCESTER' PUBLISHED IN 1757

facing p. 49

Splendide Mendax*

by H. J. Randall

THIS is not the occasion to discourse of the Art of Lying in general. The subject is too wide for any essay, and the present writer can claim no special competence. Yet, contrary to much generally received opinion, mankind for the most part are extremely bad liars,—not for want of practice, but because of inherent deficiency. This proposition is accepted as axiomatic in the practice of law and in the science of history. Few witnesses can survive a really skilled cross-examination unless they are speaking the truth or a fairly close approximation to it, and the historical witness is in little better case. The difficulties inherent in testimony are immense, but deliberate untruthfulness seldom prevails.

The causes of this grave defect in our mental constitution are want of imagination and defect of memory. It is so difficult for the ordinary, or even the exceptional man, to imagine anything beyond the narrow range of his own experience that most mendacity is merely exaggeration. By general consent the best, or at least the most consistent liars are fishermen and golfers. Yet their stories are little beyond the most obvious exaggeration ; exaggeration by expansion in the one case, and by contraction in the other. It is for this reason that an original fishing story, like that of Jonah, has retained its immortality and freshness through the long ages.

The Art of Historical Lying is not in a much better condition and its competent exponents have been few. First, as in duty bound, we must speak of the Church. The monastic cartularies contain numerous false charters, chiefly of the Old English period. They are so well executed that their historical value is only slightly inferior to that of the genuine examples. But the artistic effort required to produce a false charter is not very great. They were merely designed to supply missing links in the chain of title, a very advisable precaution when times were troubled and kings were greedy. The forms were well known and the materials were available, so that to insert a charter in the cartulary where its presence would be helpful was no great matter.

---

* Translated by Smith Minor, in one of his most inspired moments, as ' lying in state '.

The Donation of Constantine and the False Decretals were more ambitious efforts. The Donation, whereby Constantine granted to the Popes the sovereignty of Rome, Italy, and the Western Provinces when he retired to Constantinople was a fine dialectical weapon in the struggle for the Temporal Supremacy. It appeared to make the Papacy by right as well as in fact ' the ghost of the deceased Roman Empire sitting crowned upon the grave thereof '.

The False Decretals[1] were a collection of texts made in Gaul about A.D. 850. They were designed to protect the Church from violence, to abate internal abuses, and to promote the temporal supremacy. About half of the collection is forged, but the forgery is little more than tearing texts from their context, and then placing them in the mouths of the early Popes in such form as to give them force of law.

Neither of these forgeries would have deceived a critical age, and the first beginnings of historical criticism at the Renaissance disposed of both.

Of secular efforts, the Letters of Phalaris, which really owe their fame to Richard Bentley, and some of the excursions of Geoffrey of Monmouth, may rank high ; but for sustained artistry none can equal our present subject,—Charles Bertram of Copenhagen. A witty Canon once remarked that the guiding maxim of the Dictionary of National Biography was ' no flowers by request ', but even the sedate D.N.B. waxes comparatively enthusiastic over Bertram. It calls him ' the cleverest and most successful literary impostor of modern times ', and adds that ' the ingenuity and learning displayed in Bertram's forgery are really extraordinary, and fully account for the unparalleled success which the imposture obtained '.

The writer in D.N.B. cites B. B. Woodward's papers in the *Gentleman's Magazine* but he does not mention Mayor's preface in the Rolls Series edition of Richard of Cirencester (1863–9, 2v.). The latter is far more thorough, and corrects certain points in the D.N.B. which may be noted here.

1. Bertram's Copenhagen edition was actually published in January 1758, not 1757. (Mayor, p. xvii). It did not contain an ' elaborate commentary '. Bertram's notes only covered chapters 1 and 2 of book 1.

2. Stukeley did not ' adopt ' Bertram's map in the second edition of *Itinerarium Curiosum*, 1776. He died in 1765.

---

[1] A good critical account is *The False Decretals*, by E. H. Davenport. Oxford, 1916.

3. It is unfair to say that the edition of the 'Six English Chronicles' by Dr J. A. Giles (which includes the false Richard of Cirencester) was published in Bohn's Antiquarian Library in 1872 after Woodward's exposure. Originally published in 1848 it was reprinted in 1872.

4. The date of Bertram's death was 8 January 1765. (Mayor, p. CXXIV).

Bertram was born in London in 1723. His father was a silk-dyer, and in 1743 the family moved to Copenhagen in the entourage of Princess Louisa, who married the Crown Prince of Denmark, afterwards Frederick V. In 1748 Bertram obtained a licence to teach English in the Marine Academy, a school for naval cadets, but he never ranked as a professor. In addition to the famous forgery he produced several perfectly innocuous works on English and Danish grammar. He died on 8 January 1765 aged 42, and his protagonist or victim, William Stukeley, two months later (3 March) at the age of 77. As Haverfield remarks 'he had lived eight years too long'.

The story of the discovery is given in Stukeley's *Account of Richard of Cirencester*, p. 12, and also by Mayor in his preface to the Rolls Series edition, pp. CXX–CXXI.

'In the summer of 1747, *June* 11, whilst I lived at *Stamford*, I receiv'd a letter from *Charles Julius Bertram*, professor of the *English* tongue in the Royal marine Academy of *Copenhagen*, a person unknown to me. The letter was polite, full of compliments, as usual with foreigners, expressing much candor and respect to me; being only acquainted with some works of mine published. the letter was dated the year before: for all that time he hesitated in sending it.

Soon after my receiving it, I sent a civil answer: which produced another letter, with a prolix and elaborate *latin* epistle inclos'd, from the famous Mr *Gramm*, privy-counsellor, and chief librarian to his *Danish* Majesty: a learned gentleman, who had been in *England*, and visited our universities. (Mr *Martin Folkes* remembered him.) he was Mr *Bertram's* great friend and patron.

I answered that letter, and it created a correspondence between us. among other matters, Mr *Bertram* mentioned a manuscript in a friend's hands, of *Richard* of *Westminster*, being a history of *Roman Brittain*, which he thought a great curiosity: and an antient map of the island annex'd.

In *november*, that year, the Duke of *Montagu*, who was pleas'd to have a favor for me, drew me from a beloved retirement, where I proposed to spend the remainder of my life: therefore wonder'd the more, how Mr *Bertram* found me out. nor was I sollicitous about *Richard* of *Westminster*, as he then called him, till I was presented to St *George's* church, *Queen-Square*. when I became fix'd in *London*, I thought it proper to cultivate my *Copenhagen* correspondence, and I received another *latin* letter from Mr *Gramm*; and soon after an account of his death, and a print of him in profile.

I now began to think of the manuscript, and desired some little extract from it; then, an imitation of the hand-writing, which I shewed to my late friend Mr *Casley*, keeper in the *Cotton* library, who immediately pronounced it to be 400 years old.

> I press'd Mr *Bertram* to get the manuscript into his hands, if possible. which at length, with some difficulty, he accomplished : and on my sollicitation, sent to me in letters, a transcript of the whole ; and at last a copy of the map : he having an excellent hand in drawing.
>
> Upon perusal, I seriously sollicited him to print it, as the greatest treasure we now can boast of, in this kind of learning. in the mean time, I have here extracted some account of the Treatise, for your present entertainment : as I gave it to Dr *Mead*, and to my very worthy friend Mr *Gray* of *Colchester*, some time past, at their request '.

On 18 March 1756 Stukeley read his paper before the Society of Antiquaries. It was published in 1757 with the title *An Account of Richard of Cirencester*, and included a reproduction and detailed discussion of the famous map, a full account of the Itinerary with identifications of the place-names, and a plate of the specimen of the MS that Bertram had transmitted. In January 1758 Bertram published at Copenhagen an edition of the false ' Richard of Cirencester ' together with Gildas and Nennius. In 1776 a second edition of Stukeley's *Itinerarium Curiosum* was issued which included (vol. II) his 'Account' and Bertram's full text notes and map, but not his preface.[2]

At this point it may be well to explain that the present writer has been fortunate enough to acquire some interesting manuscripts of Stukeley's. Of these there are three parchment bound quarto volumes, numbered I, III, and V, so that II and IV are missing. No. I is entirely in Stukeley's writing. It is marked 'copyed by Wm: Stukeley May 1749 ', and contains the text of the ' De Situ ' to the end of the Diaphragmata or Itinerary in Lib. I, cap. 7. It seems quite evident that this was the original copy made by Stukeley from the letters received from Bertram. It has additional interest from the fact that pasted into it, immediately following the title-page, is the specimen of the original MS that Bertram forwarded to convince Stukeley. Underneath Stukeley has written the following note :—

' A specimen of the original manuscript of Richard of westminster, on velum, a large sized quarto, the map is on the finest sort of vellum.[3] there was another volume containing 22 pages, before it ; which is lost. I suppose, it was that other work which he wrote, the history of Brittain before the Roman times. it was not wrote in the manner of geoffry of monmouth, but fro(m) authentic memoirs, and therefore, its loss is the more to be lamented '.

---

[2] It may again be remarked that Bertram's notes only relate to chapters 1 and 2 of Book I. Apparently the effort to annotate the whole work could not be sustained.

[3] There is no copy of the map with these MSS.

The missing volume 2 probably contained the remainder of the text of the ' De Situ '. Volume 3 is marked on the cover ' Index ', but inside ' Comment ' on ' Ricordi Westmon '. It is inscribed 'Wm Stukeley 1749 S Geo. Qu Square'. It consists of notes upon the place-names in the ' De Situ ' and suggested identifications.

No. 5, dated 1750, is a copy of Richard's Itinerary with identifications of the places. In this Stukeley has sometimes altered his original guess. The Itinerary is followed by 40 to 50 pages of notes and observations. These might be worth printing, because they contain a large number of records of things that Stukeley had seen in various places. He might be credulous, but he was a good observer.

In addition to these MSS there is a small volume bound in morocco with Stukeley's bookplate. It contains the complete text of the ' De Situ ' in the writing of a copyist, and is interleaved with plain paper for notes. Upon these Stukeley has written headings and a few notes, but nothing of interest or value. Finally the little collection contains Stukeley's own printed copy of the ' Account ' of 1757 with a few notes and corrections in his writing.

So much for the manuscripts. In 1809 an anonymous edition was published. This contained Bertram's text and notes, a translation and notes by Henry Hatcher of Salisbury, a reproduction of the map[4], and a detailed commentary on the Itinerary by Rev. Thomas Leman, F.S.A. Some assistance from other antiquaries, including Sir Richard Colt Hoare, is also acknowledged. This is really the standard edition, because the one in the ' Six Old English Chronicles ', prepared by J. A. Giles for Bohn's Antiquarian Library in 1848 is simply Hatcher's translation and notes, and Leman's commentary on the Itinerary, without Bertram's notes or map.

Of the tract itself,—*De Situ Britanniae*—it must suffice to say that the first book contains a geographical description of our islands, with the Diaphragmata or Itinerary inserted at the end of cap. 7. The second book, which comes to an abrupt end, consists of some historical notes, beginning with the Creation. Its attraction lay in the large addition that it purported to make to our Roman names. The map, according to Stukeley, contained 100 names out of 250 ' wholly new or ill-placed by former writers '. The Itinerary contained 76 names in the same category. Stukeley's figures are difficult to check, and are

[4] The map is attributed to Stukeley and dated 1755, but it is Bertram's revised map of 1758, not the map in Stukeley's ' Account '.

not wholly consistent, but they suffice to show the general effect. His encomium would have been fully justified, if only the work had been genuine.

The fact of the forgery has been demonstrated so conclusively and fully by Woodward[5] and Mayor[6] that detailed repetition would be superfluous, but it may be desirable to refer to a few of the points.

1. The original manuscript has never been found, either among Bertram's papers or anywhere else. Bertram always maintained a great air of mystery about it. The most that he would say was that ' it came into my hands in a wonderful way with other curiosities '.

2. The only evidence of the manuscript that Bertram condescended to send was the specimen reproduced in Stukeley's ' Account ' and in subsequent editions. It purports to contain the title and opening paragraph of the ' De Situ '. This specimen, which has apparently never been seen since, is contained in the MS books referred to above. It is clear that Stukeley's plate, while fairly accurate, is not a facsimile. It would be tedious to compare the two in detail, but the original makes the fact of forgery more obvious. Not only are the letters quite unlike any genuine MS of the fourteenth century, or any other known form, but the illuminated capital, the flourishes, and the lettering itself are more clumsy in the original. In a word the reproduction has been constructed, and is not a copy.

3. The present manuscripts throw some light upon the ascription of the tract to Richard of Cirencester. Bertram had attributed the tract in the first instance to Richard, a monk of Westminster. This was a perfectly safe shot, because Westminster Abbey must have had many monks who bore the name Richard. Stukeley then pointed out to Bertram that Richard of Cirencester was a monk of Westminster, and Bertram immediately adopted the suggestion. It is quite possible that before he received Stukeley's communication Bertram had never heard of Richard of Cirencester. He could not alter the text without arousing suspicion, but he changed his own title to ' Richard Corinensis Monachi Westmonasteriensis De Situ Britanniae Libri Duo '. Bertram

---

[5] *A Literary Forgery :* Richard of Cirencester's tractate on Britain, by B. B. Woodward. *Gentleman's Magazine*, N.S. I (1866) pp. 301, 617 ; II (1866), 458 ; IV (1867) 443. Earlier communications on the forgery were printed in the *Magazine* for 1846-7, XXVI, 365 and XXVII, 377, 480 ; 1853, XXXIX, 270.

[6] *Ricardi de Cirencestria Speculum Historiale.* Edited by J. E. B. Mayor. Rolls Series, 1869, vol. II, preface.

did not know, but Stukeley did, that the genuine Richard was Ricardus de Cirencestria. The 'Corinensis' was Bertram's own invention, and Stukeley never used it.

In vol. I of our MSS, dated May 1749, Stukeley has added his own title-page beginning with the words 'Ricardi de Cirencestria'. In vol. III, also dated 1749, an internal title begins 'Ricardi Westmonasteriensis' but 'de Cirencest Monachi' has been interlined between the two words. In vol V, dated 1750, all hesitation has disappeared, and he is 'Richard of Cirencester, monk of Westminster'.

4. There are no references in the genuine Richard to the 'De Situ', nor vice versa. Bertram had completed his text before he had heard from Stukeley of the existence of Richard of Cirencester.

5. The genuine Richard never cites an ancient author at first hand. The author of 'De Situ' quotes largely from Latin writers, and even Greek. He cites works that were not known at the time, and has such powers of prevision that he can even adopt the emendations (some erroneous) that eighteenth century editors were to make in the text of Tacitus.

6. In the 'Account' of 1757 Stukeley reproduced the original map that Bertram had sent him. Then (Jan. 1758) Bertram issued his edition in Copenhagen, but in the meantime had materially improved the first draft of the map, and published the improved copy. In the *Itinerarium Curiosum* of 1776 this is attached to the reprint of Stukeley's 'Account' without a qualm or a word of explanation. Yet both are supposed to be copies of the same original. Liars indeed need good memories.

Mendacity, likewise, requires a good foundation. Thomas Wright states the argument in favour of Richard's Itinerary thus[7] :—

> 'That this Itinerary was not invented by Bertram seems clear from the circumstance that his roads, though they are not always the same as those in Antoninus, have been traced where he places them, and that their existence was certainly not known in Bertram's time'.

The sources of Bertram's information are therefore an interesting question. He could not have obtained it from personal wanderings, because he lived in a foreign country, and it must, therefore, have come from books. The answer has been given with a completeness that could hardly be rivalled in the detailed analysis of the 'De Situ' by

---

[7] *The Celt, the Roman, and the Saxon*, 1852, p. 459.

Mayor. Bertram's general method, when he ceased to follow the Antonine Itinerary, was to work upon hints, guesses, and statements in Camden, Baxter's *Glossary* and Horsley's *Britannia Romana*. His main source was Camden, with imaginative suggestions of his own thrown in. A few random examples will illustrate the method.

He invents new forms of names. 'Corinensis', from the ancient Corinium, in place of the medieval Cirencestria is one instance; and 'Cantiopolis' for Canterbury (Durovernum is the ancient form) is a Greek form added as an extra flourish. Hereri Mons for a station in the Snowdon district is taken from Nennius. Eryri is the Welsh name of Snowdon.

A favourite and easy form of invention was to place 'Ad' before either a word: (Ad Vallum, Ad Murum, Ad Fines); or a river-name: (Ad Tisam, Ad Sturium amnem, Ad Sabrinam); or before a real or assumed mountain name (Ad Alpes Peninos, Ad montem Grampium). When inspiration fails, he will even descend to 'In Medio' for a half-way house. Sometimes he has tripped badly, when using this method. Three stations are named 'Ad Fines', and one of them has been placed near the summit of the Cheviots. Bertram omitted to notice that the present boundary between England and Scotland was not a line of boundary at all in Roman times.

Bibracte was a town of the Aedui in Gaul. Camden conjectured that Bray was so called. Therefore Bertram duly adopts Bibracte as the name of Bray.

The list of 92 municipia, coloniae, civitates Latio jure donatae, et stipendiariae, was quoted even by Gibbon without suspicion. The whole system of classification has been lifted from Pliny's account of Spain in his own time, and the names have been added by Bertram as the fancy moved him.

A more satisfactory idea of Bertram's method, and likewise of his genius, will be gained by running through one of the routes in the Itinerary. Iter XI[8] has been selected as most familiar to the present writer.

It runs 'Ab Aquis, per Viam Juliam, Menapiam usque'. (Bath to St. Davids). Bertram arrived at the Via Julia in this manner:— Camden, when writing of Isca (Caerleon) and the nearby town of Newport, had quoted some verses containing the words 'Julia Strata' from Alexander Neckam, a poet of the thirteenth century. Camden

[8] *The Description of Britain*, London, 1809, p. 144.

added the comment, ' it is beyond doubt that this Julia Strata was a road '. Bertram adopts this and so creates the Via Julia. A later genius, who should be immortalized but whose name is unknown, improved upon Bertram most brilliantly. He distinguished two Via Julias, and called the South Wales coast road Via Julia Maritima, and another road going northwards through Gelligaer, Via Julia Montana. These beautiful inventions have appeared on the Ordnance Survey maps up to the present time. They really deserved to be true. The prosaic fact is that we do not know the ancient name of any Roman road in Britain, nor do we know whether they had any names.

Bertram's Iter XI has been constructed by cutting into the Antonine Iter ' Ab Isca-Calleva ' at Bath (Aquae Sulis) and then going backwards to Isca (Caerleon). From Isca Bertram again works backwards along part of the Antonine route ' A Mariduno–Viroconium ' with additions and omissions.

AD ABONAM VI, the Abone of Antoninus misplaced.

AD SABRINAM VI, a Bertram invention.

STATIONEM TRAJECTUM III, the Antonine Trajectum taken from its proper place, and transferred to the other side of the Severn estuary, with an imaginary distance from the last invented station.

VENTA SILURUM VIII, Caerwent, as in Antoninus. The mileage is the Antonine distance from Abone.

ISCA COLONIA VIIII UNDE FUIT AARON MARTYR, Caerleon. The distance is taken correctly from Antoninus. Caerleon was never a colony. The 'Aaron Martyr ' frill is derived from Giraldus Cambrensis as quoted by Camden.

TIBIA AMNE VIII[9] meaning the river Taff.

Bertram seems to have taken the name from Camden and Baxter's commentaries, or Ptolemy, and then invented a station. In Stukeley's MS notes it is identified as Llandaff in the 1749 volume, and as ' Caerdyff ' in 1750. The astonishing fact remains that we now know of the existence of a Roman fort of the Saxon shore type at Cardiff. Stukeley did not know this, much less did Bertram, but a lucky shot has hit the mark.

BOVIO XX. The position of this fort has never been identified. Antoninus gives the distance of Bomio as XXVII mp. from Isca. Bertram therefore, according to the manuscript, but not the printed reading, has divided the Antonine distance by the insertion of Tibia Amne at

[9] This distance is given as VIII in the printed copies but in the MS it is VII.

VII mp. from Isca. Camden confused numberless successors by reading the Antonine Bomio as Bovio (in which Bertram dutifully follows him) and then identifying it as Cowbridge. As Haverfield remarked ' It is a mercy that he was not attracted by the names of Oxford, Cowley, and Bullingdon to place " Bovium " on the Isis '.[10]

NIDO XV and LEUCARO XV are the same as in Antoninus.

AD VIGESIMUM XX. Bertram now involves himself in hopeless confusion. The distance from Leucarum (Lougher) is the same as the Antonine distance of Carmarthen (Maridunum) from Lougher.

Stukeley was evidently very puzzled because in the notes of 1749 he identified Ad Vigesimum as Narberth Castle but in 1750 it has become Maridunum. The correlation with Maridunum makes the distance to St. Davids an absurdity. The best explanation appears to be that Bertram overlooked Maridunum but thought that he had included it. Upon this supposition (*i.e.* that the XX mp. is the distance of Ad Vigesimum from the omitted Maridunum, and not from Leucarum as stated) the facts are rather remarkable. Richard Fenton published his *Pembrokeshire* in 1811, and for the first time recorded a small Roman fort between Carmarthen and St. Davids. It is the earthwork known as Castle Flemish, Ambleston. Neither Bertram nor Stukeley could have known of this and, as we have seen, Stukeley flounders about in his attempts at identification.[11]

The supporters of the ' De Situ ' could point to this instance of accurate knowledge previously unknown as a vindication of Richard of Cirencester and his undisclosed sources. Was it then just a lucky guess ? The explanation I suggest is this.

Bertram in all probability used Gibson's edition (1695) of Camden. There Gibson records[12] that in 1692 two shepherd boys had found a hoard of Roman coins at the entrance of the camp called the Gaer in the parish of Llanboidy. Lluyd communicated the information to Gibson, and sent him a list of 24 of the coins as well as a short description of the camp. Knowing Bertram's methods the inference is strong that this foundation was quite sufficient for the creation of *Ad Vigesimum*. The name is one of Bertram's favourite forms when he could not think of anything more appropriate. Similar forms (Ad Decimum,

[10] *Roman Occupation of Britain*, 1924, p. 70.

[11] The site is unquestionably Roman. See R. E. M. Wheeler, ' A Roman site in Pembrokeshire'. *Arch. Camb.* LXXVIII (1923), 211–24.

[12] Col. 605. Also in Gough's edition of Camden, 1806, III, 140.

Ad Sextem, etc.) occur in the Antonine Itinerary, but not, as it happens, in Britain.

Ad Menapiam xviii. Menevia was the ordinary medieval name for St. Davids. The Menapii were a North Gallic tribe. Bertram appears to have considered that Menapia would sound better than Menevia.

Of the evil influence of Bertram's forgery it would be tedious to speak at length, especially as Mayor has set out the evidence in abundant detail. As Haverfield says[13] :—

> 'The forgeries of Bertram have been taken as gospel by many subsequent writers, and they have frequently been repeated by men who knew that Bertram was a forger, but did not know that they were repeating Bertram's fiction. Almost all that was written on Roman Britain between 1757 and the middle of Queen Victoria's reign is tainted from this source. Inquirers have been set on wrong tracks, and attention has been diverted into wrong channels'.

The local historians, from John Whitaker who wrote of Manchester in 1771, until 1870 and even later, have found in the false Richard of Cirencester a mine of information to supplement the deficiency of records and to flatter parochial pride. The bearers of greater names have also fallen victims to the ingenuity of Bertram. Even Gibbon quoted the famous description of the cities in his first volume, but in the second he became a little doubtful and spoke of 'the feeble evidence of Richard of Cirencester'. Others had neither doubts nor suspicions. Gough (the last editor of Camden), General Roy, Lappenberg, Lingard and Charles Roach Smith all treated the authority of the 'De Situ' as unquestionable. Unfortunately Johnson was not interested in history, or he might have dealt with Bertram as he dealt with Macpherson, and have saved English antiquaries from masses of useless labour and still more useless speculations.

There were indeed some early doubters, like Thomas Reynolds in his *Iter Britanniarum* (1799) and T. D. Whitaker in his *History of Whalley* (1801) ; but it was not until 1827, 70 years after the publication of the 'De Situ', that John Hodgson the great historian of Northumberland, clearly denounced it as a forgery. Then in spite of Lappenberg and Lingard the volume of criticism grew, until Woodward and Mayor proved the case 'beyond a peradventure'.

At the present day our view of Bertram differs from that of Woodward and Mayor. They were righteously angry; we can afford to laugh. They were vexed at the immense harm that Bertram had done to the study of English archaeology; we can appreciate the genius that

[13] *Roman Occupation of Britain*, p. 78.

perpetrated the great joke. Woodward and Mayor, speaking with the scholarship of the nineteenth century, could say that his Latinity was of his own day, not medieval ; that his knowledge was defective, and his sources a hodge-podge. But these things were not apparent at the time.

Hatcher, who was no incompetent antiquary, becomes almost lyrical on the subject in his preface to the edition of 1809, written 50 years after 'De Situ' was first published.

> ' To the general fidelity of Richard I am happy to bear unequivocal testimony . . . If then we find him thus exact in those cases where we have the means of bringing his fidelity to the test, with what justice can we charge him with deception in the use of those authorities which have not reached our times ? . . . He aspires only to the humble honours of a compiler; he makes no mystery of his authorities; . . . he displays that unaffected candour, which never accompanies a conscious deception, and represents his pursuits as the innocent recreation of a vacant hour, but discountenanced and thwarted by his superior. We appeal to every candid and unprejudiced mind, whether a man in such circumstances, and with pretensions so modest, could have either the inducement or the inclination to incur the guilt of deception or forgery '.

Now that the forgery is apparent we can appreciate the artistic effort that created it. The able critics who exposed it gave no credit to Bertram for the skill with which the diverse sources were woven together to make a presentable whole. The beauty of the thing is nowhere more obvious than in Bertram's notes, which were not reprinted after the edition of 1809. In one place, he points out a misquotation in his own forgery, 'Errat hic Ricardus', and in another he remarks ' I do not see how our monk rushed into this wonderful calculation, as I can find nothing else like it '. Could the very breath of suspicion be more effectively blown away.

But the best evidence of Bertram's genius is the long success of his fabrication. He was no medieval forger working at a time when the written word was almost sacrosanct, but he imposed upon the age of reason itself, the period of Gibbon and Johnson and Adam Smith. It was 70 years before he was first found out, over 100 before he was effectually demolished, and his influence has hardly ceased in our time. Not until the consulate of Crawford did Bertram's names begin to disappear from the Ordnance Survey maps. One wonders if Bertram could have imagined as he worked in his study at Copenhagen about 1747 that in the twentieth century the names that he was inventing would still appear on the official maps of England. We can ignore his work, but we can salute his genius, and can say that of all historical liars he deserves most fully the title of 'Splendide Mendax'.

PLATE I

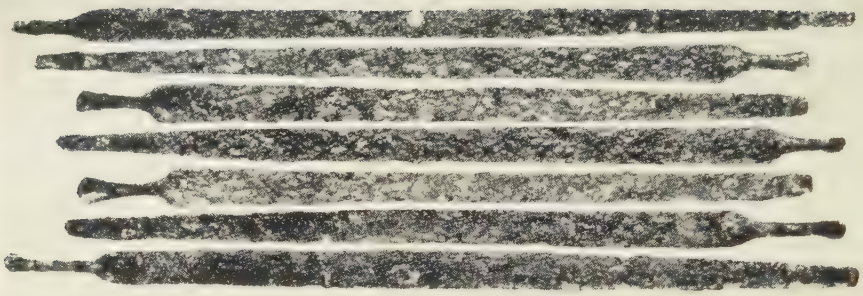


Fig. 1. IRON CURRENCY BARS FROM HOD HILL, NORMAL TYPE

facing p. 61

Currency Bars and Water-Clocks:

The Verdicts of Archaeology Reviewed

by E. Wyndham Hulme

I WAS first led to question the soundness of 'the Iron Currency Bar' theory when investigating the early history of iron-making in the Forest of Dean—the district from which it is generally accepted that these bars emanated. If the 'Currency Bar' theory is well-founded, the inference is unavoidable that the Celtic tribes who used this form of currency were a race endowed with a very low mentality—but this inference is not supported by recent archaeological research.

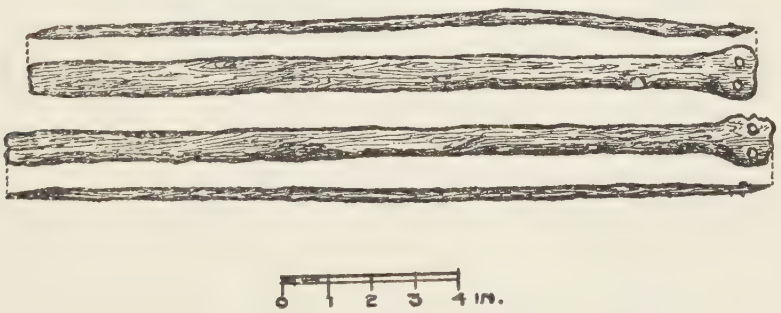


Fig. 2. IRON CURRENCY BARS FROM WAYLAND'S SMITHY, BERKSHIRE
An unusual type. For the normal type see fig. 1

Mr Hilaire Belloc has said 'the more obvious a thing is the more glory there must be in denying it', but there is nothing obvious in the above theory beyond its improbability and the unfamiliarity of its advocates with the elementary practice of smithing. For Mr Reginald Smith's theory requires us to believe that these sword-shaped bars with their rudimentary socket-handles (figure 1) are finished articles which constituted in certain parts of Britain the Celtic system of currency.

The system comprised six weight denominations ranging from the quarter to the quadruple unit, as follows :—

PRESUMED STANDARD WEIGHTS [1]

Denomination	*Grains*	*Grammes*	*Avoirdupois*
Quarter-unit –	1192	77.4	2¾ oz.
Half-unit – –	2385	154.8	5½ oz.
Unit – –	4770	309.7	11 oz.
Unit-and-a-half –	7155	464.6	16½ oz.
Double Unit –	9540	619.4	22 oz.
Quadruple Unit –	19080	1238.8	44 oz.

N.B. The most common is the double unit.

There is no correlation in these standards between length and weight, which is remarkable, for in metallic currency one expects to find increase in value characterized by some increase in its superficial area ; but here the longer bar is frequently the lighter of the two.[2]

It is clear, therefore, that we are intended to believe that the correlation lies between weight and value, *e.g.* if the double unit denomination bar is the value of a two year-old cow one of the quadruple unit would buy two. But when Mr Reginald Smith is confronted with a bar, the weight of which (11484 grains or 26¼ oz.) falls between the double and quadruple unit, he makes the following astonishing statement. ' This excess of virtue [*i.e.* weight of metal] need not disconcert us. As Professor Ridgeway [in his *Origin of Metallic Currency*, p. 262] says, all primitive peoples estimate the value of copper or iron currency by measurement rather than by weight ; and this particular specimen is just under the average length '.[3] But Prof. Ridgeway also says (*op. cit.*, p. 3). ' It is difficult to conceive any people, however primitive, employing two standards at the same time which are completely independent of each other'.

The objections to the Currency theory, other than those already brought forward, may be summarized as follows :—

(a) No similar graduated system of weight-currency based upon conventionalized forms of the same wrought material is recorded in any country.

(b) No similarly-shaped bars have been found with contemporary grave-furniture

---

[1] From *British Museum Guide to Early Iron Age Antiquities*, 2nd ed., 1925, p. 165, where illustrations of bars of 1, 2, and 4 units are given.

[2] *Proc. Soc. Antiq.* Ser. 2, XX, 1905, p. 185.

[3] Ibid. p. 190.

(c) The system is not known to have existed in the continental regions from which the Celtic tribes are known to have come.

(d) At Tiefenau, near Berne, a long-tanged sword-shaped bar is found with a quantity of early Gaulish coins.

(e) The system disappears from Britain with suspicious suddenness at a period when the possession of weapons in any stage of their manufacture would have entailed serious consequences upon the offending tribe.

(f) No tradition of buried treasure has attached to the sites where these large hoards of bars have been found.

(g) The so-called socket-handle is an impossible element in a conventionalized and finished article.

(h) The Celts at the time of Caesar's raids were in possession of many forms of tanged cutting implements. It is not necessary to assume that the bars of the six weight denominations represent six forms of early British swords.

Provisionally, therefore, it may be accepted that the Currency Bars are bars in the intermediate or interrupted stage of working which always occurs in smithing practice. As such they are not likely to have been seen or described by Caesar or to have functioned in ordinary commercial transactions, though it may have been the practice of the forges to send them out for the local smiths to finish.

Let me now turn to the subject of smithing—an art which, owing to its extensive ramifications, makes a search through its literature in quest of the generalized information required by the archaeologist particularly difficult. Landrin, for instance, in an interesting preface to his *Manuel du Coutelier*, 1835, frankly admits the impossibility of including in his work all the traditional practices of the blade-smith which even the monumental treatise of his predecessor Perret had omitted. Still, sufficient can be gathered from the literature to remove the only objections of weight which can be urged against the theory that these sword-shaped bars with pinched-in ends are tanged sword moods or moods of other cutting implements.

The hand-forger of iron or steel has from the earliest recorded times worked in stages determined by the time taken by the white-hot iron to cool to a heat at which forging becomes impracticable. These stages or heats may be grouped as follows :—

(a) 'Mooding', *i.e.*, moulding the heated bar approximately to its desired shape.

(b) Smithing, *i.e.*, drawing out and finishing the implement to its final shape and length. The effect of smithing is always accompanied by an extension of the 'mood'.

These two operations are carried out by the same workmen on the same anvil but are not continuous. 'Mooding' proceeds until a

sufficient stock of ' moods ' has been accumulated, when the ' moods ' are reheated and finished—work always beginning with the tang and shoulder of the ' mood ' and finishing with the blade. Grinding to an edge completes the process so far as the blade is concerned. John Holland, of Sheffield, who wrote the *Treatise on the Manufactures in Metal* for Lardner's Cabinet Cyclopaedia, is a good authority on the subject. Dealing with the manufacture of hand-forged forks he says (vol. II, p. 23) : ' The fork is reduced, as usual, from the rod of steel into something of the following figure (not shown here)—the tang, shoulder and shank being roughly forged, and the blade as it is called left thick and whole. In this state it is called, in common with all articles after the first formation by the hammer, a mood (mould) '.

For the benefit of metallurgists who are not archaeologists, and of archaeologists who are not metallurgists, the following data on the evolution of the Celtic sword, and the general principles of its manufacture may be acceptable.

Fig. 4, taken from von Sacken's *Das Grabfeld von Hallstatt*, pl. 5, shows the close resemblance of the Hallstatt iron sword to its bronze predecessor and contemporary (fig. 3). The contours of the iron blade must have been difficult to produce without extensive grinding. The Hallstatt sword was a luxury article. The softness of its blade and the weak union of the blade and handle must have made it an untrustworthy weapon in the battlefield. Forrer, the leading continental authority on sword making, notes the advance in the technique of sword manufacture in the La Tène period. A straight blade is substituted for the flowing contours of the Hallstatt weapon. The blade is forged thinner. The entire weapon is constructed on lighter lines, and a substantial tang runs through the grip. Forrer states that the elasticity of some of these continental blades is such that they can be twisted spirally 3 to 4 times without breaking.

Mr Reginald Smith[4] furnishes further particulars. Quoting De Reffye, from Keller's *Lake Dwellings*, he says ' the cutting-edges are not of the same iron as the body of the blade. The workman, after having forged the latter out of very tough, fibrous iron . . . welded on each side little strips of soft iron to form the cutting-edges '. Of greater importance is the statement of Vouga,[5] quoting the opinion of an expert, that one at least of the swords bearing a maker's mark ' is

[4] *Proc. Soc. Antiq.* Ser. 2, XXVII, 1914–15, p. 108.

[5] *Les Helvètes à La Tène*, p. 35.

PLATE II

Fig. 3 Fig 4

Fig. 5

Fig. 3. BRONZE SWORD (HALLSTATT). BLADE 2 ft. 3 ins.

Fig. 4. IRON SWORD (HALLSTATT). BLADE ABOUT 3 ft.
From von Sacken's *Das Grabfeld von Hallstatt*

Fig. 5. IRON SWORD (LA TÈNE), WITH PORTION OF IRON SCABBARD ADHERING.
The short sword with anthropomorphic bronze handle serves to date the former. Blade about 2 ft. 6½ ins.
From Mortimer's *Researches in Burial Mounds of East Yorkshire*

facing p. 64

PLATE III

FIG. 6

ROMAN COPPERSMITH'S WORKSHOP

A sheet of soft metal is being hammered to shape on a wooden mould. Note the round-nosed hammer. On the right a vessel is being polished by hand. A relief in the Naples Museum.

From Neuburger's *Die Technik des Altertums*, fig. 37

FIG. 7

GREEK SMITHY

From 6th cent. B.C. Attic Vase from Orvieto, on the left the hearth; above various smiths' tools, drillbow, hammers and water jug. In Boston Fine Arts Museum.

From Neuburger's *Die Technik des Altertums*, p. 52, fig. 57

made of *acier corroyé* (steel formed by hammering several strips together), which though not very hard is extremely tough and gives a very sharp edge'. Vouga proceeds: 'as the lake-dwellers were not conversant with present methods of producing steel, the blade has split in several places and is seen to consist of several layers one over the other '.[6]

English technologists will accept the observations of these Continental authorities without necessarily endorsing their conclusions. A Birmingham metallurgist, for instance, has suggested to me the following alternative solution. ' The smiths ', he says, ' learned by experience that the thinner the blade was forged the more rapidly it became hardened when heated in a charcoal fire and subsequently quenched. Consequently they hammered their edges thin, doubled and welded them together, and finally tempered them by heating and quenching '. Prof. C. Desch of the National Physical Laboratory substantially confirms this statement. He writes :—

> ' It is difficult to believe that the practice, common in later times, of welding a hard steel edge on to a soft iron back, was practised in prehistoric or very early times. It is more likely that the iron, forged into shape, was hardened on the edge by exposing it to charcoal in the fire and then quenching. The Japanese method of covering the sword with clay, removing the covering from the edge, and then heating in charcoal to carburize the edge, is a refinement of this method. That ancient swords often show alternations of hard and soft metal is likely enough when the nature of the bloomery process is considered. The heating is necessarily unequal, and whilst some parts of the mass become almost pure iron, others may retain an appreciable quantity of carbon. This is true even of modern iron made in the Swedish Walloon hearth '.

The constitution of pure wrought iron is not substantially changed by heating and quenching ; that is the property of steel. Hence unless bloomery iron contained a considerable percentage of carbon in its constitution, or was subsequently carbonized, heating and quenching would have had little effect. It is, however, improbable that the Celts anywhere left their blades untempered, though, as Vouga has pointed out, different qualities of blade were already being manufactured at this early period.

Moxon, in his *Mechanic Exercises*, 1661, refers to an old saying:

> ' He that will a good Edge win
> Must Forge thick and Grind thin '.

---

[6] Quoted by Mr Reginald Smith. *Proc. Soc. Antiq.* Ser. 2, XX, 1905, p. 108.

As this procedure would involve a waste of material without any countervailing advantage some technologists have expressed a doubt as to the truth of the tradition. But the old saying must be interpreted solely in relation to contemporary practice. It is a warning to the smith to leave plenty of metal for the grinder to work on—as would be needed in razor-grinding, surgical instruments, etc.,—where the sides of the blade were slightly hollowed by grinding. The adage therefore would have no reference to early sword making where the loss of metal would be mainly due to repeated forgings.

The earliest reference to the practice of blade-smithing is to be found in the Scholiast's note on the Argonautica of Apollonius Rhodius (v. 761), and its importance lies in the fact that it is a quotation from the lost treatise of Pytheas of Marseilles which is generally dated as *c.* 330 B.C. It runs as follows :—

> Ἐν τῇ Λιπάρᾳ καὶ Στρογγύλῃ (τῶν Αἰόλου δὲ νήσων αὗται) δοκεῖ ὁ Ἥφαιστος διατρίβειν. Δι' ὃ καὶ πυρὸς βρόμον ἀκούεσθαι καὶ ἦχον σφοδρόν. Τὸ δὲ παλαιὸν ἐλέγετο, τὸν βουλόμενον ἀργὸν σίδηρον ἀποφέρειν (*more probably* ἐπιφέρειν) καὶ ἐπὶ τὴν αὔριον ἐλθόντα λαμβάνειν ἤ ξίφος ἢ εἴ τι ἄλλο ἤθελε κατασκευάσαι, καταβαλόντα μισθόν. Ταῦτα φησὶ Πυθέας ἐν γῆς περιόδῳ λέγων καὶ τὴν θάλασσαν ἐκεῖ ζεῖν.

TRANSLATION

> In Lipara and Strongyle (these are some of the islands of Aeolus) Hephaestus is thought to dwell. Hence here can be heard the thunder of fire and violent noises. Of old it was said that any man who would bring unworked[7] iron could call the next morning and take away the sword or what else he had ordered, provided he put down the proper fee. So says Pytheas in his descriptive geography (*i.e.* the Periplus) stating that there the sea boils.

It is remarkable that this passage should have been overlooked by all historians of the iron manufacture—for it opens so many avenues to research—besides forming the basis of the Wayland legend. It is, however, to be found in Depping and Michel's *Wayland Smith* (Pickering, 1847), an excellent monograph, which in its turn has been omitted from the extensive bibliography appended to Miss K. M. Buck's edition of the Icelandic Sagas. Pytheas is, of course, a good authority. The practice which he notes in Sicily *c.* 330 B.C. indicates that the smiths already worked in two stages, for the feature in the

[7] I have translated ἀργὸν as 'unworked' on the ground that iron is never described as 'bright' in Greek writings. Its usual attribute is 'black', 'gray' or 'violet'. The description, however, 'bright or glittering' would not be altogether inappropriate if applied to meteoric iron.

legend which excited the curiosity of antiquity was the fact that the smith could produce the finished article on the following morning with the promptness of the modern undertaker. Further it indicates that in pre-Roman times the forge of Vulcan was primarily the workshop of military weapons and armour. The industrial applications of iron-forging are later developments of the blade-smith's and armourer's art—though largely based upon the same technological practice.

I now come to the well-known description of Britain in Caesar's Commentaries (v. 12). The *apparatus criticus* was set out at length by Mr Reginald Smith in 1905 with his customary ability. Hence I propose to deal only with the reading *taleis* and the question of the translation of the final phrase. The passage with its variant readings runs as follows :—

' Utuntur aut aere aut nummo aureo aut { anulis / aliis / taleis } ferreis ad certum pondus examinatis pro nummo '.

The readings *anulis* and *aliis* are from the α group of MSS, but *taleis*, derived from the β group appears now to be the accepted reading. As Mr Reginald Smith says, the reading of the text has been settled by archaeologists, and this statement at once raises a doubt whether due weight has been given to philological reasoning. The whole of chapter 12 is regarded as an interpolation, probably prepared by a military reporter for Caesar's use. The construction of the sentence is, and must remain, incorrect and inelegant, but not more so than might be expected in a narrative condensed from rough memoranda. As *aliis ferreis* is inadmissible the choice lies between *anulis* and *taleis*.

FIG. 8

Now *talea*, though a word of uncertain origin, is firmly established in Latin and in languages derived therefrom. The word must have brought before the minds of those who used it a very clear picture of its significance, viz., the long-shaped cut such as is found in notch-grafting on the scion prepared for insertion into the stock. (Fig. 8). In this sense it is applied to the short stout posts with sharpened ends, a foot long according to Caesar, employed as a defence against cavalry attack ; to the lighter stakes, sharpened at both ends, used by the charcoal-burners to form a wattle-and-daub covering for their piled loppings, and to the wands used by surveyors to obtain an

alignment. In the English tally we still find the long cut by which identification of the twin parts is secured—not the short ratchet-cuts which register the value of the account. Neither in Latin nor in the Romance languages is there any instance of the application of the word to metallic commodities. Nor can any resemblance be shown between the sharpened wooden *taleae* and the British blunt-pointed iron sword-shaped 'moods', except that both are oblong.

On philological grounds, therefore, the reading *taleis* is doubtful. Caesar, who uses it in the technical military sense as a caltrop, could never have applied it to an iron sword-blade in the intermediate process of its manufacture.

Prof. A. E. Housman has kindly furnished me with the data for the various MS. readings. His note printed below[8] explains the rejection of the *anulis* reading, for this appears only in one MS. of the α group. The reading *aliis* has much stronger support. Beyond this I must not venture to go, but for the purpose of furnishing a translation of the phrase *ad certum pondus examinatis pro nummo* I will combine the two readings and render the sense of the passage as follows, viz., that iron rings and other forms of iron were used as currency when their weight was definitely established. This version harmonizes Caesar's statement with the universal practice of the metal trades. Mr Reginald Smith, however, translates the phrase *ad certum pondus examinatis* as 'graduated according to a fixed weight standard', *i.e.*, the bar was weighed before it was mooded or smithed. I do not dispute that his translation is admissible, but it lands its supporters in difficulties which my version avoids. For how could the smith weigh a portion of his bar until he had cut it off? If he used bars of standard section how is it that there is so little correlation between length and weight? The loss of metal at every heat made the use of the balance superfluous. In other industries where scales were essential these are shown in iconography, but in illustrations of smithing practice they are not found. (Figs. 6–7). Iron was, no doubt, sold by weight. My point is that it was never made to weight.

---

[8] 'utuntur aut aere aut nummo *aureo* aut *aliis* ferreis ad certum pondus examinatis *pro nummo*'.

Aureo, *aeneo*
aliis α and 2 MSS of β, *taleis* 2 MSS of β,
anulis 1 MS of α, *pro nummo* α, omitted by β.
(Signed) A. E. H., 3 Nov. 1932.

Whether Mr Reginald Smith's identification of certain bronze vessels of culinary form with Celtic water-clocks, possibly connected with Druidic time-measuring methods,[9] is not known to the present writer. Hitherto they have been treated as culinary vessels. The resemblance will be seen by comparing Déchelette's hanging cauldron (fig. 9) with Mr Reginald Smith's examples, figs. 6–15 in the above paper. They are frequently found patched like our modern laundry coppers, and some appear to have served this purpose.

Fig. 9.
CAULDRON AND CHAIN SUSPENSION
From Déchelette's *Manuel*

Wright[10] refers to three bronze kettles now preserved in the Museum at Newcastle 'of extremely thin metal so that they would readily feel the fire . . . One of them has three feet to stand upon'. Déchelette concurs in this explanation. He says:[11] 'La tenuité des parois permettait d'y faire bouillir l'eau très rapidement'. According to the same authority these culinary vessels emanated from a common centre not yet ascertained. The trade in them was international and they are probably of Greek workmanship. Hence it is appropriate that they should be found, as at Wotton and Sturmere, carefully packed together. There is a fine example of a bronze bowl with iron collar and ring from Gärdsby, Öland, illustrated by Nihlén[12] (p. 139), but I can find no description of it in the text. Mr Reginald Smith ignores this explanation. He claims as Celtic water-clocks operated on the percolating principle all perforated vessels of the culinary type whether their walls are left of reasonable thickness or are hammered thin. It is curious that he should have adopted the percolating principle, for Dr Fleet's paper,[13]

[9] *Proc. Soc. Antiq.*, Ser. 2, xxvii, 1914–15, pp. 76–95; *British Museum Guide to the Early Iron Age*, 2nd ed., 1925, pp. 162–4.

[10] *The Celt, the Roman and the Saxon*, 6th ed., p. 403. [11] *Man. d'Arch.* iii, p. 265.

[12] J. Nihlén, *Studier rorände äldre svensk järntillverkning*, etc. (Stockholm, 1932). An English translation of this important work is greatly to be desired.

[13] *Jour. Royal Asiatic Society*, 1915, pp. 213–30.

to which he refers, is written to prove that the more ancient form of Indian water-clock was operated on the opposite principle, viz., that of emptying itself in the course of a defined space of time. The apparatus was simple, but based on scientific considerations. The quantity of water was defined and the diameter of the orifice perforated by a gold wire was determined by the specification of the length and weight of the wire.

In the second place it is doubtful whether, with one exception—a unique vessel of fairly stout bronze with straight sides and radial ribs, possibly a fat-rendering vessel—any of Mr Reginald Smith's bowls would float without some external support. Their iron collars, straps and rings would tend to make them top-heavy. They would heel over and sink in a most unphilosophical manner. The water-clock theory, however amended, appears to be hopelessly improbable. No philosophical instrument maker would have followed culinary forms so slavishly, or have indulged in such a variety of types and sizes, or have designed a gong with a frying-pan handle.

It is pleasant to find myself for once in agreement with Mr Reginald Smith. His discoveries as to the weight relations of these culinary-type vessels deserve careful consideration. His results must be accepted without reserve. His weights are given, no doubt, with proper corrections, and there is no question of ' averaging ' to leave a loophole for doubt. We may hope that his work will be extended to include the ascertained weights of foreign specimens, whether perforated or not, for if Déchelette is correct they all come from a common centre. In the meantime what explanation is forthcoming of the results obtained ? For this purpose I suggest the following provisional solution.

The coppersmith at this period of the Roman occupation of Britain bought or received his ingots cast in the scale of weights outlined by Mr Reginald Smith. By the custom of his trade he used the whole of his metal for each vessel as he had to account for it to his client, as indicated in fig. 6. With the methods he employed, viz., cold-hammering, annealing, planishing and burnishing, there would be no appreciable loss of metal. Nor would he be unduly restricted in the variety of his shapes and sizes. Vessels of identical weight could be produced widely differing in design and construction, though there would be some tendency towards standardization. In other words, the coppersmith did not cut out his circles from a rectangular sheet of copper or bronze, but worked from the ingot through the sheet to his

desired object. This solution, though offered only as a temporary explanation of Mr Reginald Smith's weight relations, appears to me not unreasonable, for it is clear that in the case of the precious metals the balance was always employed at some stage or other.

Let me now turn to the weight relations of the so-called iron currency bars. Mr Reginald Smith's standards are based upon the method of averaging—a method denounced by Prof. Ridgeway[14] in no measured terms. When the figures upon which the averages are based are examined it will be found that great latitude has been exercised in assigning specimens to a given unit-class. Amongst those, for instance, of double-weight standard (9540 grains) the extremes are 11484 grains (Hod Hill) and 6726 grains (Spettisbury). Some correction must be made, no doubt, in both instances, for the condition of the two specimens. In length the extremes are 21 and 34 inches respectively. Is it not apparent that in the double-weight class we have a short heavy dagger mood (Glastonbury, 21 inches, 9098 grains) in a crowd of sword moods, the blades of which when finished would measure from 30 inches upwards. Moreover to these classes thus liberally determined there are exceptions. Dr J. Newton Friend, a warm supporter of Mr Reginald Smith's theories, provides an instance from the Worcester Museum[15] ' the *only* well-preserved example found in Worcestershire '. The length of the specimen is 12.72 inches, the weight 1933 grains. Hence it stands about half-way between the $\frac{1}{4}$ and $\frac{1}{2}$ unit-classes.

The theory that mooded bars of sword-shape with rudimentary tangs or with perforated wings for rivetting, the weights and sizes of which so obviously indicate different designs in the finished implements, were intended for currency purposes, is one which technologists will not lightly accept.

Mr Reginald Smith's train of reasoning was probably fired by the discovery of the Glamorgan standard weight (4770 grains, or 11 oz.) and this has led him to constitute his series of unit weight denominations and to classify the bars by weight considerations only. A more rational classification, I submit, would have been to have accepted the old theory that these bars are half-manufactured articles and to have grouped them by a correlation of their length, weight and thickness dimensions—with the object of ascertaining their ultimate purpose. Herr J. Nihlén[16] describes the Swedish sword and scythe moods as

[14] *op. cit.* (in text, p. 62), p. 218.
[15] *Trans. Worcs. Naturalists' Club*, 1918–22, pp. 103–4.
[16] *op. cit.*, p. 207.

'eine Art halbfabrikat welches anscheinend als Münzen zur Anwendung kam'. He occupies, therefore, a middle position—but his acceptance of these bars as a form of currency is surely weakened by the fact that they are 'mit Schneide versehenen'. The circumstances under which they have been found in Sweden correspond almost exactly with those ascertained in this country. I do not suggest that with my proposed classification it would in all cases be possible to identify the moods with their final form—but with the Bigbury and other finds of iron goods of all descriptions it would be a comparatively simple matter to allocate the great bulk of them.

To resume—the objections to the old theory that these bars of sword-shape are half-manufactured tanged cutting implements have been shown to be groundless. It has always been the practice in forging to accumulate moods before proceeding to smithing. In smithing there is always a loss of metal proportionate to the number of heats to which the mood is subjected. Finally if the moods were bought up by merchants or convoyed by swordsmiths to the local centres this was done because it was a more convenient arrangement than finishing them at the forges. The careful packing of the moods, point to socket, in boxes or crates to avoid injury in transit, suggests that they were buried in much the same condition as they were sent out. When committed to the ground the risk of detection was small. The Celtic tribes looked forward to the recovery of their freedom in the near future. As their hopes waned, the secrets of their burial sites died with the undertakers.

In concluding this somewhat polemical article may I express the hope that Mr Reginald Smith will recognize that some of his theories are of an extremely provocative character and need a correspondingly vigorous treatment. The indebtedness, however, of all metallurgists to him for his laborious investigations, extending over a quarter of a century, in ascertaining the distribution, dimensions and weights of these moods and vessels is beyond all question. Whatever theories as to their purpose may in future prevail their foundations will be found to have been laid upon the exact measurements which we owe to him and for which we are profoundly grateful.

My acknowledgments are due to Major E. A. Marples for valuable help in preparing the account of smithing practice; to Prof. C. Desch for permission to print an extract from one of his letters; and to several members of the Newcomen Society for kind suggestions and advice.

Triticum dicoccoides
Part of ear and two spikelets

T. dicoccum
ear and spikelets

T. durum

T. vulgare
different types

facing p. 73

The Origin of Cultivated Plants

by A. E. Watkins

School of Agriculture, Cambridge

MOST developments of civilization demand a settled system of agriculture, and are not compatible with nomadic life. The exact connexion between the rise of civilization and the rise of agriculture is possibly uncertain. The cultivation of different plants developed in different places: rice in the East and wheat in the West, for example. The cultivators may have attained only a low level of culture, and thousands of years may have elapsed before they produced a civilization of which the marks have endured. But to know where the different species of cultivated plants originated must help to trace the origins and diffusion of civilizations.

In studying this problem three methods have been used: the linguistic and historical; the examination of seeds found in prehistoric remains; and the search for regions where the wild relatives of modern crops occur. Vavilov* has developed a new method, which depends essentially upon collecting the varieties of each crop and finding regularities in their geographical distribution. For two reasons his conclusions can best be illustrated by wheat. First, because this is one of the most important crops, which he has studied in great detail. Secondly, because much has been learned about the origins and relationships of the various forms from genetical and cytological research—that is to say from their behaviour when crossed and the way in which their differences are inherited.

In England, nearly all varieties of wheat are similar in type. They differ only in minor characters, such as colour of ear or grain; and form part of a single species, *Triticum vulgare*. Only one, Rivet or Cone wheat, is unlike the rest; and this belongs to a distinct species, *T. turgidum*. But if the whole world is searched many thousand

---

* Papers on the origin and distribution of cultivated plants have been contributed by N. I. Vavilov to the *Bulletin of Applied Botany* for 1926, 1927 and 1931. His paper on 'The problem of the Origin of the World's Agriculture in the light of the latest Investigations' is published by Kniga (England) Ltd., Bush House, Aldwych, W.C. 2.

varieties will be found, and the differences between these are often very great. When grouped according to the degree of their resemblance, they fall into about twelve species, within each of which they are more or less similar. Some of the species, in their turn, are more alike than others ; and the whole twelve are easily divided into three groups, as follows :—

Group I	Group II	Group III
T. aegilopoides (wild form)	*T. dicoccoides* (wild form)	(no wild form)
T. monococcum	*T. dicoccum* (Emmer)	*T. vulgare* (bread wheat) and its near relatives *compactum*, *Spelta*, and *sphaerococcum*, of small importance.
	T. durum (macaroni wheat)	
	T. turgidum, *T. polonicum*, *T. persicum* (not very important)	

The first group need not be considered. The only cultivated species, *monococcum*, is grown in mountainous parts of Europe, chiefly for forage, and it is doubtful whether it has ever been connected with the cultivation of the other sorts.

Genetics and cytology have shown that the origins of these three groups, and probably of the species themselves, were distinct, occurring at different times in different places. In plants, chance hybridization between related species is not uncommon ; and the immediate result is sometimes a new species, which breeds true from the first and gives no outward trace of its derivation. There is little doubt that this was the origin of the second wheat group; but the actual parents—probably some wild grasses—have not yet been identified. It is unlikely, on the whole, that the cultivated species of the group came from the wild *dicoccoides*; probably they all arose independently in the way described. The species of the third group came in like manner; and the evidence suggests that one progenitor of each belonged to the second group.

The principal conclusions to be drawn are that the two groups, and very likely the species composing them, originated suddenly, and independently; and that the forms that gave rise to them may not be easy to identify. The third group is probably more recent than the second.

Wheat is an Old World crop. Its introduction to America, Australia, and South Africa, has occurred only in the last few hundred years. On botanical evidence, Emmer should be the most ancient cultivated species, since the ear of this form breaks up when ripe and the grain is closely held by the chaff, as in wild grasses but not in the

other cultivated wheats. This conclusion agrees with other evidence. The species is only grown today in scattered patches, chiefly by isolated tribes who cherish their ancient customs. It is grown by the Basques in Spain, by certain peoples of south Germany, Russia, and Abyssinia; it occurs here and there in India, and has been found by Vavilov in some Armenian settlements in Persia. But archaeological evidence shows that in the past it must have been widespread. In England it was probably the only wheat in pre-Roman times. In Egypt it was cultivated at least from the Badarian period onwards; and is the only wheat so far found in the tombs, with the possible exception of a single sample from Fayum, doubtfully identified as *T. vulgare* but alone among Egyptian finds in being carbonized.

Vavilov has collected wheat wherever it is grown, and worked out the distribution of the varieties, especially those belonging to *durum* and *vulgare*, the most important species. The greatest number of *vulgare* varieties was found in Afghanistan, in the eastern districts especially, and fewer and fewer were discovered as the distance from this centre increased. In Persia, Baluchistan, the Punjab, and Bokhara, the number was still large ; in England, Siberia, and eastern China, small. The general trend can be illustrated by grouping the varieties according to colour of chaff and grain, by whether they have rough or smooth chaff, bearded, half bearded, or beardless ears, and so on. It is then found that all the classes so formed, about sixty, occur in Afghanistan, and nearly as many in neighbouring regions ; but far fewer in those that are more remote. This is clearly shown by the following short list :—

Afghanistan	59	Baluchistan	46
Persia	52	India	32
Caucasus	20	Mongolia	31
Italy	12	China	18
Belgium	6	Manchuria	12

This result is very striking. Nor is it confined to wheat. As Vavilov says ' in spite of the internationalization of cultivated plants, the wanderings of the peoples, in spite of colonization and the antiquity of agriculture, it is still possible to establish . . . areas of definite endemic varieties and races, regions displaying a maximal primary diversity of varieties, and to find out series of regularities in the distribution of these varieties '. The explanation suggested is that the centre of diversity of a species is the place where it originated.

T. vulgare, therefore, came from eastern Afghanistan. Here it would have had the longest time to evolve new forms. In the outer limits of the area it has been present a much shorter time ; few new forms will have been evolved ; and few forms will have penetrated from the place of origin.

One other third-group species, *compactum*, also came from Afghanistan. Though rare elsewhere, it is cultivated there in numerous varieties. A second, *sphaerococcum*, is confined to northern India. The distribution of the remaining species, *Spelta*, is imperfectly known, and may be widely different from that of its relatives.

The results for the second group species are quite different ; as would be expected from the genetical and cytological evidence that the groups had separate origins. For the primitive *dicoccum* there are naturally few data. Its present distribution is a mere relic of the past. But the centre for the rest of the group, considered as a whole, is undoubtedly to be found in Abyssinia and the Mediterranean basin. Vavilov believes that the most important species, *durum*, came from Abyssinia, where it is very diverse—much more so than in Egypt and Algeria, the two countries previously thought to be richest in *durum* varieties. To the north of the Mediterranean there are, as would be expected, fewer forms than to the south; and from the high diversity in Egypt there is a steady falling off in the direction of Syria, Mesopotamia, Persia, India. The other species of the group are not very important. *Polonicum* is grown locally in the Mediterranean basin. The wild *dicoccoides* is found from Palestine to the Caucasus and Persia. *Turgidum* is nowhere very common, but ranges further than its relatives. While most diverse on the northern shores of the Mediterranean, centreing perhaps in Greece and Dalmatia, it is found here and there in many countries from England to Western China. Only *persicum*, which is botanically rather distinct from the other species of the group, has a more distant centre, being almost confined to Georgia and Armenia.

The Mediterranean area, then, is the centre of the second group. *Durum* probably arose in Abyssinia, and is now the staple species of all north Africa, from Egypt to Morocco. The most widespread species of the third group, *vulgare*, is almost a stranger to the area. Though not uncommon there today, it is a recent introduction of European colonists ; except perhaps for a few imperfectly studied forms from the oases of Algeria. It occurs, also, in Abyssinia ; but the varieties show clear affinities with those of Portugal, and are found only in regions subject to European influence.

Map showing Centres of the Origin of Cultivated Plants.

Centres from which various cultivated plants have spread.
The dotted lines unite into groups the principal centres.

If Vavilov is right, the second and third groups came from widely distant centres ; and they are believed, on very different grounds, to have arisen separately. The centres of both groups—Abyssinia and Afghanistan—are mountainous areas ; but other mountainous regions, such as the Alps, are poor in varieties, and other crops have different centres. Though widely grown in Afghanistan, barley is there very uniform; despite the fact that wild barley, *Hordeum spontaneum*, is an abundant weed.

Within the last few years the Russian workers have collected the varieties of many other crops. Not all species have been completely studied yet, and there are still parts of the globe imperfectly explored, but it has been possible to state the place of origin of most important crops, and to point to the existence of several primary world centres of agriculture. The conclusions are provisional to some extent, but show clearly the general tendencies. Sometimes the varieties of a crop are distributed like those of wheat ; and, as in wheat, there may be several species which have originated in different places. Cotton, for example, has two groups of species, the Old World and the New, and two centres have been found for each. Sometimes a different plan is followed; the most striking being the so-called secondary crops, which are believed originally to have been weeds in primary crops. One is rye, which, over the vast areas of north Europe and Asia where it is chiefly grown, is comparatively uniform in its botanical characters ; and is only found in any diversity in southwest Asia, where it has little or no importance as a cultivated plant. In these regions the cultivation of *T. vulgare* originated; and winter rye occurs, in many forms, as a weed of winter wheat. Rye is hardier than wheat, and on the slopes of the Hindu Kush it gets more and more abundant in the fields until, at a height of 2000 metres, it supplants the wheat entirely and is grown as an independent crop. Vavilov supposes that as *T. vulgare* spread from south to north rye was carried with it as a weed. At the northern limit of wheat cultivation, the weed supplanted the crop, so that rye alone was grown; but it occurs in few varieties, its centre of diversity being still Afghanistan, the home of the parent crop.

The same principle is believed to apply to oats and some other cultivated plants. In melons, the gradual entry into cultivation is clearly seen. Wild melons, besides growing on uncultivated soils, may invade the fields of wheat or cotton. In India and Africa they are then

sometimes collected and used as food ; and from this it is only a step to the deliberate selection and cultivation of the best varieties.

So far, Vavilov is inclined to distinguish five principal world-centres.

The first of these is Afghanistan, especially southeast Afghanistan below the Hindu Kush. From here have come bread wheat, beans, peas, lentils, carrots, pomegranates, and other species. The standard of agriculture is generally primitive; and the varieties, in their coarseness and poverty, contrast with those of other places such as Egypt, Asia Minor, Abyssinia.

The second is in southeast Asia, but its exact position is not yet known. It has produced naked oats, naked barley, millet, soy beans, many fruit trees and cultivated species of Cruciferae, and probably rice.

The third and fourth centres are the Mediterranean and Abyssinia; but until the latter has been properly explored their exact relationship to one another is not quite certain. Between them they have given rise to several kinds of oats and peas, to large seeded flax, many vegetables, and other crops. Abyssinia produced *T. durum*, hulled barley, and a series of other plants.

Finally, a fifth centre is to be sought for in the New World ; but so far little is known about this region, and there may indeed be two or more centres, independent of each other. Mexico, Peru, and Chile, are the homes of the potatoes, Jerusalem artichokes, maize, tobacco, and the New World cottons, as well as many other crops.

It will be seen that, in several plants, allied forms have sometimes arisen at different times in far distant places. In the case of wheat, and some others such as oats and cotton, this agrees with genetical evidence. In barley, the existence of two centres is more surprising and no genetical basis is known for this at present.

These regularities in distribution, found by Vavilov, are very striking. He has given a simple explanation, which broadly speaking must, in my opinion, be accepted. It must be agreed that there were separate origins for the second and third wheat-groups, one in north Africa, one in southwest Asia. The only doubt is whether the centre of greatest diversity defines the place of origin exactly. Regularity in the wanderings of peoples may have helped to bring about an orderly dispersion of crop varieties, and may have shifted the centre of diversity from the place of origin. The wealth of forms in southeast Afghanistan may have been increased by the frequent migration of peoples into India. Such a process would not have given two different centres for

wheat, one in Abyssinia and one in Afghanistan ; but though it might not greatly alter Vavilov's conclusions it will have to be allowed for when the subject is attentively considered.

If the distribution of varieties were carefully followed it is possible that definite evidence could be found for the movement of peoples from one place to another. A critical study of the forms of *T. durum* shows that very often the place they came from can be told from their appearance ; and when comparisons are made it is not surprising to find a similarity between the forms of Morocco and Spain, or Greece and Crete. Sometimes, closely similar forms can be traced over wide areas, and this would probably be caused by the movements of the tribes that grow them. For example, an apparently identical series, usually known as *Shutur Dandan*, has been obtained from Iraq, Meshed in Khorasan, Seistan, Zhob in Baluchistan, and Sind; while a similar form occurs in the Punjab. In Zhob, where it is grown to neutralize the effects of the evil eye in a field, it is known as *Mecca Muazzama ghanam*, and is considered so sacred by the Mohammedan cultivators that it cannot be sown without the ceremonial ablutions necessary for the offering of prayers.

Vavilov has clearly shown that the varieties of cultivated plants have an orderly distribution ; that regularity prevails where chaos might have been anticipated. In the simplest case each species has a definite centre, believed to be the centre of origin, in which it is most diverse ; and each becomes progressively more uniform as this centre is left behind. In other instances the distribution is less simple, but more or less plausible reasons for the complications have been given.

Furthermore, the species themselves appear to have come from a small number of limited areas ; and these, it is suggested, are primary world-centres of agriculture. It is possible that some of Vavilov's conclusions will, in course of time, need modifying. But in general they agree well with the simple series of regularities he found, and no theory of the origin and spread of human cultures can safely ignore them.

Notes and News

A PAGAN SURVIVAL IN PHOCIS

In March of 1932 Mr D. A. Garnsey (of New College) and myself broke our journey from Athens to Delphi at the monastery of Holy Luke of Stiris. While we were there a shepherd from Distomo came in to shelter from a storm. He brought with him his shepherd's crook, carved as most such crooks are, but with a device so curious that I made a drawing of it. Later I made a plasticine model from the drawing, and from this model the accompanying photographs (plate 1) were taken.

The upper part of the crook, made of smooth yellowish wood, followed in outline the usual shape—a stout rounded handle, and a downward curved projection forming the crook for catching sheep. It was this part which bore the carving : the head and bust of a woman, the body below the breasts turning into two snakes, which swept round in a circle so that their mouths met her breasts. Her arms were bent at right angles ; with her hands she held the snakes just below their heads, and was evidently suckling them. Round her neck was a collar or wide necklace, with some pattern on it of wavy lines. On her head was a flat, plain circular head-dress, with two wings hanging straight down at the sides and hiding the back part of the head. On the tympanum round which the snakes were coiled was carved, apparently, a skull. The head and neck of the figure were joined to the handle of the staff ; the rest stood clear. The whole was carved from one piece of wood. The design was decorative and ingenious, and though the carving was not over-skilful, the finish was good.

The shepherd told us that the crook had been carved for him in Distomo, but more information he was unable or unwilling to give. Possibly he was embarrassed, in the presence of the abbot, by the clearly pagan character of the carving. But he was evidently very proud of it, and pleased at the interest we displayed.

The quality of the carving was of less importance than the subject, which seems to be an interesting survival of paganism—the Mother-Goddess suckling her snake-son in his theriomorphic form, attached to the crook as a charm to bring fertility to the flocks. This is not

impossible or even unlikely. Mr J. C. Lawson records[1] that Demeter still survives in the country districts under the name of δέσποινα, and here she seems to have taken a definite shape.

The district is remote, and was formerly surrounded by centres of Chthonian cults. There were the Muses on Helicon. There was a cult of Ge at Delphi before Apollo arrived there, and the serpent was too powerful to be abolished in one single conflict. The altar of Ge remained close to Apollo's sanctuary, and long after he was firmly established there oracles still came up from the Earth.[2] Apollo himself sometimes advised his suppliants to sacrifice to Ge.[3] And at Levadia, a day's walk from Distomo, the old worship of Trophonios was never overlaid with invading deities : indeed it probably succeeded to much of the prestige of the old cult at Delphi.

When Pausanias visited Levadia, Trophonios was associated with the river-nymph Hercyna. Their statues stood together, and beside them stood staffs with snakes coiled round them.[4] The main attraction was a prophetic serpent in a deep underground cavern—a Chthonian deity in its most primitive form. Farnell says :[5] ' in the shrine of Trophonios at Levadia there was a prophetic snake that had to be propitiated with offerings of honey-cakes ; and it is very probable that Ge herself was one of the aboriginal powers of the Trophonion, and only became supplanted by her young " double " Hercyna, whose badge is the snake '. The prophetic Trophonios would naturally attract most of the attention, and Ge, by the time she was completely anthropomorphized, had dwindled to the status of a mere Nymph.

At the same time the presence of Ge in some form certainly remained at Levadia. Pausanias says[6] that those who went down into the oracular cave sacrificed not only to Trophonios and his sons, but to Apollo, Cronos, Zeus Basileus, Hera Henioche, and to ' Demeter, whom they surname Europe, declaring that she is the nurse of Trophonios '.

The sentence might very well be the answer to the problem ; the local carving showing the particular local legend : Demeter nursing

---

[1] J. C. Lawson, *Modern Greek Folklore and Ancient Greek Religion*, p. 90 ff.

[2] Cf. Euripides, Iph. in Taur. 1260.

[3] *e.g.* an inscription recording an oracle to the citizens of Cyzicus. *Bull. de Corr. Hell.* 1882, p. 455.

[4] Pausanias, 9.39.3.

[5] L. R. Farnell, *Cults of the Greek States*, III, 10.

[6] Pausanias, 9.39.5.

Trophonios in his theriomorphic form. The presence of two snakes, though in any case required by symmetry, has also a symbolic significance, for Trophonios was closely associated with Hermes. Part of the initiation before consulting the oracle was to be ceremonially washed by two youths known as Hermai.[7] A funeral inscription bearing the name of Hermes is found actually in Levadia, where Trophonios was lord of the underworld.[8] By the time of Cicero they were identified.[9] Now ' this proto-Hermes was always a Snake-God, and before the era of complete anthropomorphism he was thought of in snake-form. But it is an essential element of his function that he was not a single snake . . . but the double snake, male and female, the most prolific form of copulation in the animal kingdom '.[10] Also ' in the minds of his worshippers, the character of fertility must be closely connected with the character of a Chthonic deity '.[11]

Trophonios was in any case χθόνιος, and identification with Hermes would confirm his native tendency, as the Nourishing God,[12] to be ἐπιμήλιος. And it gave him the right to appear as two snakes, the form in which the carving shows him. No shepherd could imagine or desire a more potent fertility-charm than this most fertile of all gods being nursed by the mother of all fertility. To place the god and goddess on his crook—the very keystone of his relations with the flock—would be the surest way of passing on their properties to the sheep and goats.

It is unusual to find a god thus portrayed in his animal form, and remarkable to find him coupled with an anthropomorphized goddess. Most probably the goddess and the snakes are joined only by the exigencies of the crook's shape ; but if it be held that the goddess actually terminates in a serpent, certain interesting comparisons may be made. One Attic black-figure vase-painting shows four semi-serpentiform nymphs beneath a vine. Dr Böhlau points out[13] that they are kindly helpful nymphs, clearly contrasted with the ravaging

---

[7] Pausanias, 9.39.7.

[8] Collitz. *Dialect. Inschr.* I, 455.

[9] Cicero, *De Natura Deorum*, 3. 22.

[10] A. L. Frothingham, ' Hermes, Snake-God, Caduceus '. *American Journal of Archaeology*, XX, 175.

[11] F. J. M. de Waele, *The Magic Staff or Rod in Graeco–Italian Antiquity*, p. 31.

[12] Farnell, *op. cit.* III, 281.

[13] Philologus, LVII, 513, and plate.

goats on the other side of the vase. Miss Jane Harrison further explains[14] that both these nymphs and the snaky Erinyes are subdivisions of the Earth-Mother, specially personified for particular functions. Certain bronze vase-handles from Delphi[15] show the same S-shape as the crook and bear heads presumably prophylactic ; and another black figure vase[16] bears on its handle the head and shoulders of a woman, whose arms end in serpents' heads and are laid along the lip of the vessel in an attitude evidently protective.

It is a long way from Phocis to Babylon, but since Professor Frothingham finds a Babylonian ancestry for Hermes it may not be irrelevant to quote from him a cuneiform inscription, saying of the mother-goddess Belit-ili that ' her breast is filled with milk and the lower part of her body is that of a serpent '.[17] In Babylon, too, it was the custom for everyone to carry a stick bearing on the tip some flower, figure, or other symbol;[18] and possibly it was this kind of stick which, when introduced into Athens, was called Persian and frowned on by the older and more conservative Athenians '.[19]

None of this accounts for the face carved on the tympanum. If it is a skull, I do not pretend to explain its presence, which in this context would seem to be disastrous. Most probably it is, as Professor A. B. Cook suggests, an ordinary prophylactic face, and the resemblance to a skull purely accidental. ROGER H. ELLIS.

STEATITE VASES FROM KISH

In view of the discovery of cultural relationships between the ancient civilizations of the valley of the Indus, Elam and the ' Fertile Crescent ', it was interesting to read the notes by Mr Ernest Mackay in ANTIQUITY* reporting the discovery at Mohenjo-daro of a ' fragment of a steatite vase bearing exactly the same intricate and very unusual pattern as a double vase of steatite found at Susa in association with other objects of the Second Period of that site. That the vase was an importation from Elam is rendered the more certain by its being a

[14] J. E. Harrison, ' Delphika '. *Journal Hellenic Studies*, 1899, p. 205.
[15] Fouilles de Delphes : Monuments figurés, figs. 296, 301, 303.
[16] British Museum B. 620. *Journal Hellenic Studies*, I, plate.
[17] A. L. Frothingham, *op. cit.* p. 186.
[18] Hdt. I, 195.
[19] Daremberg et Saglio, I, 639.
* September 1932, pp. 356–7, with plate.

greenish-grey steatite, of which it is the only piece that has been found in the Indus Valley excavations '.

Mr R. Martin, who has been studying the Kish material in the collections of the Field Museum of Natural History, Chicago, recently brought to my attention a fragment (plate II, fig. 1) of a greenish-grey steatite vase which was found during the season 1929-30 in trench C 7, at a depth of six metres. Two other similar steatite fragments were found during the season 1931–32 ; one (fig. 2) from trench C 8 at a depth of six to seven metres, and the other (fig. 3) from the surface of trench C 10. It is probable that this piece was found at the same depth, but its exact location could not be recorded. The C trenches lie to the northwest of the great temple of Nabunidus, which, together with the two ziggurats and other monuments, form the great complex of mounds known as Ingharra which dominates the eastern part of the city.

Upon close examination, the Kish fragments bear the same remarkable and intricate pattern as those figured from Mohenjo-daro and Susa. From archaeological evidence, the level in which two of these fragments were found belongs to the period *c.* 2800 B.C. This date agrees very favourably with those given by M. de Mecquénem for Susa II, and by Mr Mackay for the lower levels of Mohenjo-daro.

The discovery of these three examples of steatite vases of similar designs from Kish, Susa and Mohenjo-daro adds yet another link in the chain of cultural evidence for the close interrelation between these early civilizations in the early part of the third millennium before the Christian era.

HENRY FIELD.

THE CELTS IN THE MIDDLE AGES

The minds of the readers of ANTIQUITY have been turned of late towards the Celts. Mr Christopher Hawkes has disentangled for us the different elements in the Celtic settlements in Britain. Mr Kendrick has reft the hanging bowls from the Saxons, who loved them so well that they tried to take them to heaven, and has restored them to their Celtic designers ; and Dr Mortimer Wheeler has shown that the Celts only produced their best work in a state of society such that it could afford sufficient security to make a fairly large measure of personal liberty possible, and sufficient money to give the craftsman adequate material compensation for his labour. It is as though we had been seeing the picture through the Celtic twilight and the grime of ages ; and now the cleaners have come, let in the sunshine and begun to

remove the dirt from the canvas so that the original colours can show clearly. But the process is not yet complete.

The writer, with the emerging Celts in mind, was spending a day at Wells, first to see the fine house in the Close, where, thanks to the generosity of Mr W. Wyndham, the Early Iron Age finds obtained from Wookey Hole by Mr Balch are now housed, and secondly to look once more at the Cathedral. The affinity between the curvilinear ornament on the pottery and the sculptured curvilinear foliage of the Cathedral was immediately obvious. (Plate III, fig. 2).

To anyone interested in the ornament of the medieval churches the development from the stiff and conventional, or even grotesque though handsome Norman decoration, to the riot of beauty displayed in the thirteenth century must be surprising. Something was evidently added to the stew to produce the consommé. No doubt the improved technique due to increased use of the chisel rendered the change possible, as it accounted for the great increase in the undercutting of the mouldings; but there was a change in spirit as well as in craftsmanship.

Now France can show an abundance of thirteenth century foliage, especially on her finer churches, but, handsome as it is, it is not the curvilinear foliage of England. The French work is stiffer and more stereotyped, of a style suggestive of celery, for the stalks are an important part of the design, sometimes at the expense of the leaves. (Plate IV). This is quite obviously akin to the Corinthian capital, with its volutes and upstanding foliage, and so is naturally evolved from the Romanesque capital which in France, especially in Burgundy and the South, has such close affinities to its Roman prototype. When the French foliage ceased to be stereotyped it became naturalistic, thus foreshadowing the work of the fourteenth century, such as in England can be seen in perfection on the walls of the chapter house at Southwell; or else developed a crinkly pattern known in the French guide-books as 'curly greens' (*choux frisés*).

But the English curvilinear foliage is not like any of this. Stiff-leaved is a poor name for it; stiff-stalked would be better, for though the leaves keep a certain formality, they are never stiff. Like the French examples, its evolution from the Norman capital can also be traced, but through rather a different line. As Mr Gardner has pointed out,[1] the Norman cushion capital became, by division, the

[1] *English Gothic Foliage Sculpture.*

scallop. The scallop became the incurved, or pollard willow type of ornament, presenting small, flat surfaces that invited decoration. (Plate III, fig. 1). This was applied, and sometimes took the form of trefoils in low relief, which, themselves, occur in Norman and Saxon work. The thirteenth century trefoil refused to remain flat. At Enmore church (Som.), it can be seen actually emerging from the flat into the round, and after that there was no going back. In this and in other ways curvilinear foliage was created. The volute as well as the trefoil and even the Norman version of the palmette and acanthus pointed in the same direction, but the new invention left its origins behind at once. In the larger churches, such as Wells or Romsey, the style seems to have leapt to life fully formed. It is astonishingly uniform in many buildings from about 1180 to 1250 or later. This might be expected in important churches, but the persistence of the style in equal perfection in remote and unimportant country ones is significant of the wide spread of the impulse that informed it. At Westbury-on-Trym (Bristol) it can be seen in an elementary stage, buds as it were, while at Worcester even the tail of the bird in one of the spandrils blossoms forth into trefoil-foliage. These leaves, on capitals, always grow upwards from the neck, and then cascade over in steep curves, and are generally trefoil in pattern. This love of the trefoil and of steep curves is very suggestive of the way in which the Celts adapted the classical patterns to their own use in the earlier periods. If a flat space has to be filled, the quatrefoil may be used, but the actual foliage will be almost entirely trefoil. Dr Wheeler has shown that the distinction of Celtic art from Classical and Teutonic lies in the use of 'eccentric curves and swelling forms', while he adds that 'two objects with identical Celtic decoration are of the utmost rarity'. Flowing curves and swelling forms are the principal motif in English curvilinear ornament, and its most remarkable characteristic is its uniformity of style combined with intense individuality of detail. Nothing is repeated exactly, and herein lies one great difference between English and French work of the same period, and English work of this and all other periods. Another Celtic characteristic is exhibited by this ornament, for though it suggests natural forms, it is never naturalistic. In the work of the succeeding century we can distinguish unerringly the leaf of the oak or maple or speedwell, but thirteenth century foliage grows from a tree never seen on earth.

The earlier periods when the expression of the Celtic genius was at its height have been defined as between the first and third centuries A.D.,

and again between the sixth and ninth, with an interval of three hundred years. The great period of curvilinear foliage was the second half of the twelfth and the first half of the thirteenth century, so that again a gap of three hundred years separated the periods of Celtic artistic dominance. The date of this re-emergence was not fortuitous. To quote Dr Wheeler once more :—' The golden periods of art have a solid bullion basis. Art demands both wealth and security ', which of course depend upon the consolidation of political authority. The latter part of the twelfth century was the first time that these conditions were fulfilled in England after the tenth century. During the Danish raids, and even under the Danish kings, prolonged security was a rare condition, for the strong rule of Canute came between two far from settled periods. Edward the Confessor, in his turn, cannot have controlled his earls sufficiently to make life stable for the bottom dog, and the reign of the Conqueror must have been a time of unrest and readjustment, followed by the miseries which led to the murder of the ' Red ' king. The reforms of Henry I were soon swamped in the Civil War—' when God and his Saints slept ', so that it was not until the reign of John that security and therefore comparative prosperity dawned once more for the multitude of lesser folk in England. During this time there was a renaissance in learning and art, beginning in the reign of Henry II and born to some extent of his marriage with the heiress of Guyenne, who brought to the north her heritage of the undimmed culture of the south. The revival or rather the emergence in architecture of the fine transitional work of the second half of the twelfth century, merging into the glories of the thirteenth, are the fruit of this association. When this opportunity arose may not the Celtic aptitude or tradition, still existing in many villages of England amongst a people now to a great extent sprung from both Celt and Saxon, have found expression? Certainly in the purely Celtic districts when fine building was attempted, the same style exists, but this is comparatively rare, as the Celts were not great builders, and it may be that, as was the case with the escutcheons, it was only under the executive Saxon predominance that the best and most characteristic Celtic work was produced.

It must not be imagined that the Celtic genius failed to express itself during these dark intermediate centuries. The magnificent manuscripts and the Irish crosses are there to contradict any such assertion, but the cloister alone provided the security which was necessary for its existence, and it was, for the time, driven from the turbulence of the ordinary world.

PLASTICINE MODELS OF HANDLE OF SHEPHERD'S CROOK, DISTOMO, GREECE
The first shows the attachment to the staff
(*See* p. 81)

facing **p.** 88

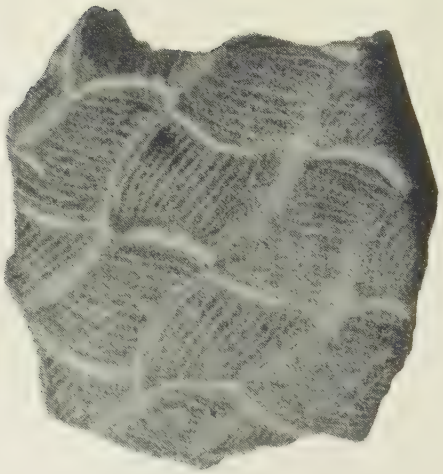


Fig. 1

Fig. 2

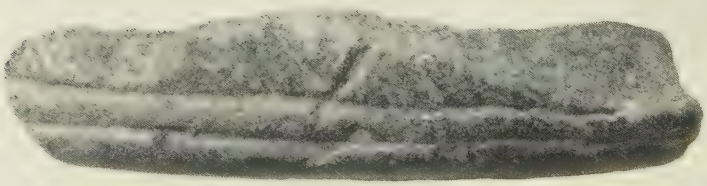


Fig. 3

FRAGMENTS OF THREE STEATITE VASES FROM FIELD MUSEUM—OXFORD UNIVERSITY JOINT EXPEDITION TO KISH, MESOPOTAMIA. (*See* p. 84)

Ph. Field Museum of Natural History

FIG. 1. POLLARD WILLOW CAPITAL, WITH FLAT TREFOIL ORNAMENT
SHERSTON CHURCH, WILTS

FIG. 2. TYPICAL ENGLISH CURVILINEAR FOLIAGE, WELLS CATHEDRAL
(*See* p. 85)

TYPICAL STIFF-LEAVED FOLIAGE, WITH NATURALISTIC LEAVES BENEATH,
LE MANS CATHEDRAL, FRANCE

PLATE V

FIG. 1. ANCIENT EGYPTIAN BRUSH FROM AMARNA (14TH CENT. B.C.)

(*See* p. 90)

FIG. 2. MODERN BRUSH FROM MAJORCA

PLATE VI

PITS AT BETTESHANGER COLLIERY, KENT, FOUND 1923

(*See* p. 90)

PLATE VII

PORTION OF THOMAS OF ELMHAM'S MAP OF THANET (A.D. 1414), NOW IN THE LIBRARY OF TRINITY HALL, CAMBRIDGE

(*See* p. 92)

PROBABLE SITE OF THUNOR'S PIT (1) REVEALED BY AIR-PHOTOGRAPHY

Crown copyright reserved

What was the fate of the curvilinear ornament? It vanished utterly, to be succeeded by the naturalistic foliage of the fourteenth century, which in its turn became stylized, but with the rectangle rather than the trefoil as the basis of the design, in the well-known decoration of the fifteenth century.

The curvilinear impulse was short-lived, but so were the other Celtic impulses at their best, and it may not be too fanciful to trace in this lovely expression of artistry the same genius that impelled the La Tène craftsmen to produce engraved mirrors and the escutcheons of hanging bowls.

DINA PORTWAY DOBSON.

MINERAL COAL IN ROMAN BRITAIN

It does not seem to be generally known that mineral coal was used in Britain in the Roman period. When there must have been a practically unlimited supply of wood for fuel in districts so far removed from any coal-field as Wiltshire, for instance, it is curious that it should have been worth while to carry such a heavy and bulky material as mineral coal over these long distances. The means of transport was probably pack horses.

Among the references to the discovery of mineral coal in definitely Roman association are the following, which though suggestive enough, does not pretend to be an exhaustive list of finds.

' Common bituminous coal ' found in the Roman villa at Baydon, on the borders of Wilts and Berks. Specimens of this coal are in the Museum at Devizes. *Gentleman's Magazine*, 1866, pt. 1, 335.

' Traces of the use of bituminous coal in the houses of Uriconium (Wroxeter) have already been met with '. *Ibid.* 1857, pt. 1, 625.

' Fragments of fossil coal ' reported to have been found in the Roman villa at Foxcote, Bucks. *Ibid.* 1843, pt. 1, 303.

In Bruce's *Roman Wall*, 1914, pp. 51–52, 145, it is stated that coal seems to have been worked by the Romans at Benwell, near Newcastle, and that nearly a cartload was found in one of the guard-rooms at Housesteads on the Wall.

Coal pits apparently of Roman age at Werneth, Lancashire, are referred to in Ward's *Roman Era in Britain*, p. 10.

Dr Mortimer Wheeler refers to fragments of coal found on the site of a Roman building at Ely near Cardiff, and to others said to have been found under an undisturbed Roman pavement at Caerwent, and in Roman strata at Chester and near Flint. *Prehistoric and Roman*

Wales, p. 268. He refers to Solinus, in the 3rd century A.D., speaking of the use of coal at Bath, as follows :—

> ' In quo spatio magna et multa flumina, fontes calidi opiparo exculti apparatu ad usus mortalium: quibus fontibus praesul est Minervae numen, in cujus aede perpetui ignes nunquam canescunt in favillas, sed ubi ignis tabuit, vertit in globos saxeos '.

Among the Wiltshire sites on which coal has been found are the villa at Stanton St. Quentin, the Roman building at Nuthills near Bowood, a rubbish heap on the Romano-British settlement close to Silbury Hill, and the Romano-British villages of Stockton and Knook. The coal on these sites very probably came from the Somersetshire coal-field, or possibly from the Forest of Dean. A note with special reference to sites in Wiltshire is printed in *Wilts Arch. Mag.* XLV, Dec. 1930, p. 170. M. E. CUNNINGTON.

TWO BRUSHES

During the recent excavations of the Egypt Exploration Society at Amarna, a paint-brush (plate V, fig. 1) was found. It belongs to the 14th century B.C. and may even have belonged to Akhnaton himself. Our purpose in publishing it however is to show its extraordinarily close resemblance to the small brushes still made and sold in the Balearic Isles. The one here illustrated (fig. 2) is $9\frac{3}{4}$ inches long and made of the leaves of the palmetto, a palm-like plant that grows wild on all rocky uncultivated ground in the islands. The ' hairs ' of the brush are formed by splitting the leaves, and the binding is of the same material. One plant alone supplies the whole of the raw materials. The Egyptian discovery proves that the modern Balearic brushes have a pedigree going back at least 3200 years ; actually they are probably much more ancient still. The same is probably true of the Balearic baskets, which closely resemble those of northwest Africa and Egypt.

Thanks are due to the Egypt Exploration Society for allowing the photograph to be published (fig. 1), and to the Assistant Secretary for her help in obtaining it from the Cairo Museum. The other (fig. 2) is from a Majorcan specimen in my own possession (and use). O.G.S.C.

PREHISTORIC PITS IN KENT

Whilst excavating for the surface lay-out of Betteshanger colliery in 1923, a polished flint axe was found in the bottom of a shallow valley. It was buried beneath about 3 feet of brickearth and adjacent was the remains of a fire, a small piece of stained earth, with pieces of charcoal and a few weathered oyster shells. The axe is now in a

museum in Scotland. A careful lookout was kept on later excavations, but although many thousands of cubic yards of earth were moved no other remains of interest were found.

During excavations for the foundations of water ponds and winding-engine houses, several man-made pits were discovered. These were dug in chalk, and were overlain by 4 to 5 ft. of undisturbed brick earth. The photograph reproduced (plate VI) shows three pits exposed in the sides of the excavation for the water pond. They are about 8 feet deep (measured from the Chalk surface) and about 8–9 feet across as seen in the excavation. The sides were sharply defined and the pits were obviously filled in before weathering had set in.

As far as could be seen the pits were circular, and though two are separated by a wall of chalk only some 2–3 feet thick, they were not connected. The infilling consists of chalky rubble, stones and earth and has every appearance of having been thrown in, and not deposited by natural causes. The brickearth cuts across both the undisturbed chalk and the infilled pits with a sharp line of demarcation. It is typical of that found in the district, being a buff, homogeneous loam with a few scattered stones, calcareous below and decalcified near the surface as shown by the darker colour. The lenticular mass of loam and stones over the chalk wall may be a more gravelly brickearth, or it may be artificially laid material similar to that in the pits.

When excavating for the winding-engine house, a larger prehistoric pit was seen. Measured from the chalk surface it is about 7 feet deep and about 20 feet long. In this case the pit has only been partly filled in with rubble which occupies the floor and is banked up a little against the sides. The interior is filled with brickearth continuous with the overlying sheet. A few bones, not identified, together with a portion of a horse's skull, were found at the bottom of this pit.

No other pits were exposed on the site although a very large area was uncovered.

The excavated surface level of the Colliery site is 73 A.O.D. and the area today is 4 miles from the coast line at Deal.

The photographs (and others not reproduced here) were taken in 1923 and shown recently to Mr H. G. Dines of the Geological Survey, who recognized their significance, and it is with his assistance that these notes have been written.* G. C. H. NASMYTH.

* In a letter to the EDITOR, Mr Nasmyth adds :—'I examined the debris carefully in 1923, when excavated ; there wasn't a scrap of pottery or even a worked flint—just chalk rubble and natural flints mixed with brickearth '.

THUNOR'S PIT

A curious story relating to boundaries comes from Kent ; it was set down first in writing in 1414 by Simeon of Durham, but the account here given follows Thomas of Elmham, whose story is illustrated by the oldest surviving English plan with any claim to be called cadastral. It represents the Isle of Thanet. The story begins at Eastry with the murder of Ethelred and Ethelbert, great-grandsons of Augustine's convert King Ethelbert, and the time is somewhere between A.D. 664 and 669. The father of these two boys was heir to the throne of Kent ; but he died before the King his father, and his younger brother Ercombert succeeded. At the death of Ercombert in 664 one of the two boys should have become king, but the throne was usurped by their first cousin Egbert. Shortly after his accession Egbert connived at the murder of his two cousins by his *praefectus* Thunor. Such crimes provided an obvious opportunity for blackmail ; and his cousin Dompneva was quick to avail herself of it, prompted by Theodore, Archbishop of Canterbury. Dompneva was wife of the King of Mercia and sister of the victims. She and the Archbishop demanded compensation, Dompneva stating that she intended to found a monastic institution and wanted a grant of land to endow it. Egbert offered her land in Thanet—' as much as her tame doe (*cerva sua domestica*) could traverse in a single rush (*uno impetu*)'. The King and his court assembled to watch, and the doe was released on the seashore at Westgate near Margate. It followed a remarkably geometric course, making several right-angle turns;[1] but about midway it encountered the wily Thunor, who may be supposed to have owned land thereabouts. Thunor attempted to head it off, when, lo and behold both he and his horse were engulfed in the lower regions with Dathan and Abiron. 'In that spot', continues Thomas, 'there is visible to this day [1414] a pit called "Thunor hys lope" [Thunor's leap]. Here I shall set down a plot of the position and shape of this island [Thanet], with the run of the doe demarcating the boundary between the domains'. At this point in the medieval record there follows the map (plate VII). He thus explains it : ' In the above plot the run of the doe is shown

[1] These actually represent the course of the boundary between the arable fields of the parishes of Garlinge and Acol, before its alteration in recent times. The course marked by Thomas of Elmham is very accurate and may be plotted from his map to the 6-inch map without difficulty. It may be conjectured that this boundary-line had been in dispute, and that it was itself the real cause of the map being drawn.

by a green line, coursing to and fro; it is three feet wide and is perfectly preserved without any break. The royal roads of the island leading from one parish to another are shown by a red line. The green line in the west portion near Sarre, near the church of St. Giles, marks the domain of the barony, later transferred to the domain of S. Mildred. The space of ground enclosed by the tame doe contained in length and breadth 48 *aratra*. Thunor's pit, once called Thunor's lope, is now known locally as Heghisdale; it is to be seen near the doe's run, against Aldeland'.[2]

The original map of 1414 is still preserved in the manuscript, a handsome book of large folio vellum leaves, now in the library of Trinity Hall, Cambridge. Reproduced several times, the best reproduction is a coloured facsimile in the Rolls Edition of the manuscript.[3] The original was lent recently to the Ordnance Survey when the map was photographed. (Plate VII). The site of the pit can be determined within fairly accurate limits from the map itself. It lies close to Manston Aerodrome; with the object of seeing whether it would be revealed by air-photography, I asked the authorities to photograph the area. The result shows one mark closely resembling the oval mark on the map of 1414. Last summer I investigated the spot, but was unable to detect anything but the faintest suggestion of a depression on the site of the oval dark mark (1) on the photo. There is however, such a slight depression visible. Adjacent to it is another, much more plain, corresponding to the round black mark (2) on the photo. Either of these may represent Thunor's pit, but both in position and shape the oval one is more suitable. (Plate VIII).

The whole neighbourhood abounds in chalk-pits; in many of them a series of galleries has been hollowed out. One such series at Alland Grange, $\frac{1}{4}$ mile S.E. of Thunor's pit, is now used for growing mushrooms. There are other pits at Cheesman's Farm and Mount Pleasant, the latter bearing the suggestive name of Quendele on Thomas of Elmham's map. It is probable that these now *open* chalk-pits represent the site of dene-holes which, after collapsing, were still used as quarries. The existing galleries at Alland Grange appear all to be modern; but

---

[2] Now Alland Grange.

[3] *Historia Monasterii S. Augustini Cantuariensis*, by Thomas of Elmham; edited by Charles Hardwick, *Rolls Series*, 1858. The account quoted is on pp. 207, 208. On the facsimile (*Historia*, frontispiece) Thunor's pit is the *oval* dark mark; the round mark is merely a grease spot (now removed) which has been unintelligently copied.

it is not unlikely that remains of a genuinely ancient one, now perhaps unidentifiable as such, may exist here and there. References to Thanet dene-holes will be found in the *Archaeological Journal* 1881, XXXVIII, p. 395 (' the caves at Manston were originally dene-holes ') ; and other references to Kentish dene-holes in the same article. One called Hegdale pit[4] in Preston near Faversham (O.S. 34 S.W. Lat. 51° 18′ 21″, Long. 0° 55′ 7″ E) was probably one of these referred to in general terms by Camden (edn. of 1587, p. 199) ; the name is interesting as recalling Thomas of Elmham's Heghisdale (see also Hasted's *Kent* II, 717).

Camden explained the dene-holes by quoting Pliny, and his explanation may be accepted (for details see my *Air Survey and Archaeology*, H.M.S.O., 2nd edn. 1928, p. 6). Thanet and the whole of northern Kent was quite densely inhabited in Roman and pre-Roman times ; and there is Caesar's own evidence for the existence in Kent of Celtic fields. Marl-pits would therefore be expected on *a priori* grounds, and Thunor's pit may safely be regarded as such. It would be interesting to test the legend by cleaning it out ; the work would be costly, but no dene-hole would be better worth examination.

The religious foundation which was built and endowed with the land was that of Minster near Ramsgate. The legend may have helped to keep alive the knowledge of the monastery's bound-marks—always an important matter in the mapless Middle Ages, and particularly so when, as here, most of the land was open arable, where natural land-marks were scarce and all in danger of deliberate removal or destruction. In the days before Boundary Commissions unconventional methods may often have been employed, if we may trust in folk-lore ; the legend of the foundation of Carthage, told also of Caistor in Lincolnshire, will be recalled. But I know of no other instance of the employment of a lady's tame doe. O.G.S.C.

MAZE SYMBOLISM

Mr G. D. HORNBLOWER writes :—'With reference to Mr W. F. J. KNIGHT'S paper on Maze Symbolism, in the December number of ANTIQUITY, is it not possible that the root *tro* (or *dru*) which it has been suggested may underlie the name of the Roman mounted game *Troia*,

[4] I am indebted to Captain M. W. Hilton Simpson, of Sole Street, for this identification ; the name will be restored at the next revision of the large scale O.S. maps of the district.

may be also traceable in the name *Trôs*, given to the mythical founder of Ilion? The city was founded, if current economical theories of history are to be trusted, as a key-fortress to maintain mastery over the valuable trade-route between Asia and Europe, and this mastery, and not the beautiful Helen, was the real prize of the Trojan war. The city may have had, beside its name Ilion, an epithet denoting its guardianship of an important entryway, derived from the same root. The root being lost, the epithet would be misunderstood and from it there might be developed, by an inverted deduction, the name of the mythical founder, *Trôs*—a process not unusual in the workings of the human mind'.

PHOTOGRAPHS FOR REPRODUCTION

A correspondent writes:—'In the editorial notes in ANTIQUITY for December last you found reasons for complaining of the poor quality of the photography so frequently used in the illustration of scientific printed matter. You are not alone in this respect for many of the photographs employed can only give an uncomfortable feeling to careful photographers. My object in writing to you is to say that if you should know of anyone who would care to accept help in this respect I shall be most pleased to give that help to anyone who may be living in the London area. In making this offer may I also say that I do not wish for payment of any kind'.

We feel sure there must be some who would gladly avail themselves of this generous offer of help from an expert photographer who has retired from active work, and if any such wish to do so letters addressed to PHOTOGRAPHY, c/o ANTIQUITY, 24 Parkend Road, Gloucester, will be forwarded.

ESHER RESEARCH STUDENTSHIP

Two years ago the Trustees of the London Museum, through the generosity of Viscount Esher, instituted the Esher Research Studentship in memory of Reginald Viscount Esher, one of the Founders of the Museum. The Studentship is normally tenable for two years, and is awarded for the purpose of promoting research into some aspect of the history or archaeology of London, whether by documentary research, by excavation, by museum-work or by a combination of these methods. The award is made by the Trustees on the recommendation of an advisory committee which includes representatives of the

Universities of Oxford, Cambridge and London, the Society of Antiquaries and the British Academy.

The first Student was Mr G. C. Dunning, B.SC., who during the past two years has been carrying out research into the history and archaeology of the neglected subject of medieval pottery in this country and the adjacent parts of the Continent, with special reference to the large collections in London. The results of Mr Dunning's researches will probably in due course be published by the Trustees of the London Museum.

In March 1933 the Studentship will be re-awarded, and applications from candidates possessing either an approved degree from a recognized British or foreign University or some equivalent qualification are invited on or before 11 March 1933. Particulars may be obtained from the Secretary, the London Museum, St. James's, S.W. 1.

THE PEDIGREE OF THE KEESHOND

All interested in dogs and their ancestry will have read the article in the December number of ANTIQUITY written by Dr Max Hilzheimer, the German savant, and translated by Roland G. Austin of Glasgow University. To Keeshond breeders there are two passages of great interest. First, the illustration (fig. 9) of the Assyrian war-dog. Unlike most of the other dogs illustrated it has a long coat and a certain resemblance to the heavy type of Keeshond. I may add the resemblance is in outline only and not in character, for the dog in question has hold of an intruder or enemy in no uncertain fashion!

I have upheld the idea for a long while that the Keeshond is very probably the original 'friend of man', the first *dog* in fact—the watch-dog that enabled him for the first time to enjoy the blessings of deep sleep. Before he had his dog to warn him of danger he could never sleep more soundly than an animal. The dog that he bred—from the wolf it seems—was his companion and watch-dog. It had the sensitive prick-ears of the wild species (the wolf, the fox, the hyena, etc.), the thick coat of the North, and that curly tail which is a characteristic thousands of years old. (One wonders what it came from? Pride in being the first animal to be tamed by man?)

It may not be generally known that the Bushmen of Australia, one of the most primitive of living peoples, keep dogs as watch-dogs, but on the reverse principle to ours. These dogs are trained to range in the bush ahead of their master and to return to him at once *silently*

A YOUNG KEESHOND NOT YET IN FULL COAT
('The prick-eared, curly-tailed dog')

facing p. 96

on the approach of danger, *i.e.* of a man of another tribe. They are valued very highly by the Bushmen, and their methods of raising delicate members of the litter would not appeal to us ! They are said occasionally to eat them, but their principal function is that of watch-dog against attack by enemies.

We know that the Keeshond was exactly like the present type in 1740, for we have the little Keeshond model at the end of the silver spoon of that date made at Nymwegen. You used a spoon of this type if you were a supporter of the patriot Cornelius de Witt, and the dogs are called after him Cornelius (Kees) dogs. Just as in England, if you were a partisan of the Regent (afterwards George IV), as opposed to the partisans of George III, you had chairs with the Prince of Wales' feathers introduced into the decoration—a subtle way of showing with whom your feelings really were.

In 1740 then we have pictorial evidence that the Keeshond was just like he is now. Does anyone know of a picture or reference to the Keeshond between 1740 and the days of Julius Caesar, who in his *De Bello Gallico* refers to the Canis Pomeranis with prick ears and curly tail ? I wish it might be possible to bridge the preceding centuries back to the Mesopotamian seal of 3,000 B.C. on which Dr Hilzheimer states (p. 417) that ' there is an undoubted representation of a Pomeranian ; this by the way is the only illustration of a dog of this group [the fox-terrier and his close relation the terrier] known to me from ancient Mesopotamia '. I can only imagine that the cousins of the Keeshond gradually assumed the traits most required of them—the elkhound a herd dog, larger and faster ; the Mongolian sheep dog, thicker and woollier; the Huskie, heavier shouldered for pulling work; and all the dainty evolutions of the breed: the Shipperke, the British Pomeranian, the Chien Lou Lou and the Maltese terrier.

G. WINGFIELD-DIGBY.

Recent Events

The Editor is not always able to verify information taken from the daily press and other sources and cannot therefore assume responsibility for it.

Excavations have been made at Whitehawk Camp, near Brighton, and Dr Cecil Curwen writes that the outstanding features are :—

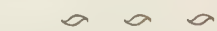
(1) Two large post-holes guarding a causeway, with two smaller ones behind them in the line of the rampart.

(2) A pit of beaker date outside and close to the 3rd ditch, with a spread of beaker and (?) Peterborough pottery across the silting of the ditch *well above* the level of the neolithic occupation, and separated from it by a thick band of sterile fine silt.

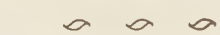
(3) Two adult and one infant inhumations in the neolithic occupation level, and contemporary with it.

(4) A complete sheep's skeleton at the bottom of what looks like a post-hole on a platform jutting into the 4th ditch.

(5) A considerable amount of pottery, etc.



'The Gertrude Bell Memorial Expedition to Arpachiyah near Nineveh'. We have received a circular asking for subscriptions (to be sent to the Expedition, British Museum). We quote from it: 'Arpachiyah is a prehistoric settlement about 200 miles north of Baghdad, 4 miles east of Nineveh . . . the only known site in Northern Mesopotamia which exhibits remains of the fifth millennium B.C. on the surface over a wide area'. The surface indications are promising, and we shall look forward to interesting results later on.



The Slindon raised beach has been examined by Mr J. B. Calkin (of Wychwood School, Bournemouth) and a full report is promised. Its relative age is of critical importance in determining the antiquity of man in southern England (*Nature*, 26 November 1932, p. 813).

NOTES AND NEWS

Captain T. W. M. Johnson, R.A., writes (from India) :—' I have noticed in South Staffordshire and in the "Black Country" that there is a weed with a conspicuous red flower which grows especially on old slag-heaps and the sites of ruined iron-works. I am informed that the name of this plant is Rose Bay Willow-herb. It appears that this plant grows better on slag than on other soil. I thought that this fact might be of use in helping to find the sites of Roman ironworks or slag-heaps '.

We suspect that willow-herb will seed and grow on *any* freshly turned soil, however, not specially on slag-heaps. It is an abundant weed on the reverted arable of the North Hampshire downlands, especially where woods have been cut down.

Mammalian remains of Ice-Age are reported from Tunstead Quarry, near Buxton. Bones of bison, giant Irish deer, and of red deer have been identified. (*The Times*, 16 December, p. 9).

The expedition organized by Major Bagnold in the southern part of the Libyan Desert has spent two months investigating the geology, botany, entomology and archaeology. Dr K. S. Sandford of Oxford University has been engaged in the archaeological survey. (*The Times*, 30 November, p. 11).

Under the direction of Mr V. Nash-Williams excavations were in progress last December on the site of the Roman legionary fortress at Caerleon, Monmouthshire. The excavation has been more particularly in the centre of the fortress but included the site of the southwest gateway and part of the defences. A report is printed in *The Times*, 27 December, p. 16.

In order to determine the date of the walls at Chichester a trench was cut through the earth bank, which is faced on the exterior with masonry. Pottery of various periods was found but owing to circumstances it was not possible to make a conclusive exploration and it is hoped to make another cutting which will give more precise evidence. (*The Times*, 27 December, p. 16).

Mousterian deposits, deeply stratified, have been found in the caves at Athlit, where Miss Garrod is conducting excavations for the British School of Archaeology in Jerusalem and the American School of Prehistoric Research. Remains of hippopotamus and rhinoceros and Levallois flakes, with a skeleton resembling *Palaeanthropus palestinus* found at Athlit last year, were discovered in the third layer. (*The Times*, 19 December, p. 11).

A Saxon spearhead found in the Thames near Boulter's lock, Cliveden, has been deposited in Reading Museum. (*The Times*, 16 December, p. 12).

A 'Woodhenge' at Caebetin on the Kerry Hills, Montgomeryshire, has been excavated by Mr Noel Jerman of the University of Wales and Mr R. U. Sayce, of Oxford University. Evidence shows that the circle of wooden posts was about 19 feet in diameter. From pottery associated with the burial which was found the site is dated as Middle Bronze Age. This is the first instance of a wooden circle in Wales. (*The Times*, 24 December, p. 13).

A Roman mosaic pavement, of fine design and workmanship, has been found at Nyon, on the Lake of Geneva. (*The Times*, 22 December, p. 15).

Various expeditions are arranging to continue work in Egypt this season. The Egypt Exploration Society will be at Tel-el-Amarna, and in conjunction with the Oriental Institute of Chicago will resume the recording of the bas-reliefs at the Temple of Abydos. The French Institut will again make the tombs of Deir-el-Medinet, near the Valley of the Kings, its principal study. (*The Times*, 22 December, p. 15).

In the course of digging on the site of the Pewterers' Hall in Lime Street, City of London, interesting evidence of a Roman roadway was revealed in successive layers of gravel for a depth of eight feet. There was nothing to indicate a paved surface. The lowest layer was a brick-earth stratum, which has been generally met with in the City area of Roman times. The roadway was more than 30 feet wide. (*The Times*, 15 December, p. 11, with plan).

Illustrations of the Greco-Roman city of Apamea in Syria, showing mosaics, the main avenue, and the ruins of the temple of Bacchus were printed in *The Times*, 31 December. The excavations are under the direction of Professor F. Mayence, of Louvain University, from whose information a detailed report was printed in *The Times* for 30 December, p. 9.

A systematic survey of British hut-villages in West Cornwall is being pursued by Lieut.-Col. F. C. Hirst, who has already found evidence of ten sites in the parish of Zennor (near St. Ives) and observed indications of others, though no actual remains can be seen. It is now proposed to survey the adjoining parish of Morvah. (*The Times*, 20 January, p. 17).

A report on a bronze axe-head found by Mr Guy Brunton at El Matmar, south of Assiut in Middle Egypt, and dated as Middle pre-Dynastic (*c.* 4000 B.C.) is printed in *Nature*, 22 October. The axe-head has been examined by Mr H. C. H. Carpenter, of the Royal School of Mines, for its structure, analysis, and hardness, with some very interesting results. The report includes illustrations of the microstructure of the axe.

Excavations conducted at a stone circle at Old Keig, Aberdeenshire, by Prof. V. Gordon Childe have yielded sherds of pottery of Hallstatt period. (*Glasgow Herald*, 13 December).

A number of Egyptologists have come to an arrangement whereby drawings of all pottery found will be sent to one centre and, when sufficient have been collected, will be issued in the form of *corpora* covering the whole range of Egyptian pottery. Cards have been printed and are supplied to excavators. On one side of the card is pasted a drawing or print of the pot, and on the other are a series of headings under which is entered all the information required by the Editor of the *corpora*.

This should make a valuable contribution to scientific archaeology and it is gratifying to note that the list of Egyptologists not participating is short. The Egyptian Antiquities Service and all Nationalities working in the country are represented. Anyone interested can obtain specimen cards and information from the Editor, Mr G. L. Harding, 200 Euston Road, London, N.W. 1.

Articles of special archaeological interest in recent numbers of the *Illustrated London News* are :—

Discoveries at the necropolis at Vounous, in Cyprus, relating to the worship of the Snake-God, a Bronze Age Cult. 10 December 1932.

Stone Age art of Scandinavia from rock-carvings at Vingen, north of Bergen, Norway. 17 December.

Maya religious art in Guatemala. 24 December.

Reproductions of the drawings in the British Museum of early Christian fresco heads from the catacombs, including the possibly prototype portrait of Christ, and a head of St. Paul. 31 December, with an article on the drawings by Rev. C. C. Dobson, who also wrote one for *The Times*, 24 December, p. 9.

Illustrations of the Tutankhamen shrines, now in Cairo Museum. 7 January 1933.

Discoveries in Ithaca by the expedition promoted by Sir Rennell Rodd, during which considerable finds of votive and other pottery were made. 14 January.

Pottery, jewelry and weapons from Damghan, on Tepe-Hissar, in North Persia, where a succession of cultures illustrate conditions of life and art in Iran. 28 January, with notes by Dr A. Upham Pope, who also describes the finds in an article in *The Times*, 28 January, p. 6, which includes particulars of the several Expeditions at present excavating in Persia.

Some remarkable sculptures have been found at Persepolis, the capital of the Achaemenean kings of Persia, by the University of Chicago Expedition which is under the direction of Dr Ernst Herzfeld. They include a series of wall relief-sculptures illustrating in great detail incidents in court life, as well as inscriptions of historical interest. Particulars are printed in *The Times*, 25 January, p. 9, and 4 February, p. 9, with illustrations on p. 14.

Reviews

STRASBOURG-ARGENTORATE : préhistorique, gallo-romain, et mérovingien. *By* ROBERT FORRER. *Strasbourg, Librarie Istra*, 15 *rue des juifs*, 1930. Two vols., *pp.* 884, 610 *illustrations and* 154 *plates.*

' It is gratifying, but inconvenient ', says Dr Forrer, ' to live in historically momentous times '. The printing of these two large volumes was begun in 1913 and only finished in 1927 ; various kinds of paper, ink and type are found in their various parts, and, of course, what was printed under a German government was printed in German, what under a French government, in French. One cannot blame the author if the volumes are somewhat forbidding in appearance. The reader who will attack them resolutely will soon forgive their faults and admire their merits.

Prehistoric and Merovingian Strasbourg only occupy a few pages at the beginning and end : the vast bulk of the work concerns the Roman site. As becomes an author in Dr Forrer's position, he has relied mainly on two classes of material : excavations, with their evidence concerning structures, and finds. The book is really a *corpus* of these two types of material. This is what makes it ponderous and unwieldy in bulk ; but the material is so interesting in itself, and so admirably treated, that its full publication is more than justified. At the same time, Dr Forrer himself recognizes that there is room for a much smaller book on Roman Strasbourg in which the material here detailed should be digested and the conclusions to be drawn from it set out in simple terms.

Of systematic excavation, little can be done on so ancient and crowded a site. But Dr Forrer has let slip no opportunity of looking into whatever holes and trenches have been dug for many years past, and noting their archaeological significance—the kind of work which we have only lately undertaken in London : and he here reproduces scores of sketches and stratification-diagrams made in the course of such observations. The chief outcome is a remarkably full general account of the ancient fortifications. An Augustan auxiliary fort remains somewhat conjectural, in spite of some relics of what may be its cemetery ; but we are on firmer ground with the earth-and-timber rampart of Legio II Augusta, early in the first century A.D. This legionary fortress, evacuated when the legion went to Britain in 43, was later tenanted by various units and detachments, and there is some evidence of a stone rampart-wall added during this period. A new phase begins with the coming of Legio VIII Augusta, about A.D. 80 ; this legion added a revetment of stone to the earth bank, entailing the filling-up of its ditch and the digging of a new one further out. Unfortunately this wall is not dated ; Dr Forrer is prepared to place it anywhere from the late first century to the late third ; but the brick-stamps in its lacing-courses seem to the present reviewer rather early than late in that long period. Lastly, a new wall studded with frequent towers is added, built on to the outer face of the old one : it is largely made of re-used material, and contains timber frames such as are found in our Saxon Shore forts ; it follows the outline of the old fortress-wall except at one part where it projects slightly beyond it, and Dr Forrer thinks that its builders did away with the earthen bank that lay behind the earlier wall. Its construction is again undated : but it belongs to the familiar series of late towered walls which we are in the habit of ascribing to the end of the third, and to the fourth, centuries.

The same careful observation of detail has resulted in the recovery of much information concerning the street-plan inside the fortress and the *canabae* outside it. Good houses with excellent frescoes have come to light ; there is much interesting detail about house-construction ; there are temples, 'giant-columns', cemeteries, riverside quays, etc. The plotting of coin-finds gives evidence of a reduced population in the fourth century, when the suburbs were deserted and the people took refuge within the walls.

About half the book is an illustrated catalogue of finds, grouped under such headings as coins, potters' stamps, weapons, evidences of industry, sculpture, etc., treated with a completeness and a degree of learning that could be expected only from a very able archaeologist and the curator of a great museum. It is inevitable that, in such a work, readers should find themselves differing from the author here and there on points of detail or detecting minor inaccuracies ; but to enumerate such things would serve no useful purpose. A reader lays the book down with the feeling that he has been personally conducted through innumerable trenches by a guide who calls his attention to every significant feature and allows him to draw his own conclusions, and then through the cases and galleries of a great local museum by the same guide—always pointing out the significant thing, but never thrusting forward his own interpretation of it. At the end, one feels that one knows Strasbourg: that one's ideas about it are one's own ideas, got from a first-hand study of the evidence ; and that one knows not only the place, but the learned and courteous and self-effacing guide who has achieved this extraordinary feat of demonstration. R. G. COLLINGWOOD.

EUROPE AND CHINA : a Survey of their Relations from the earliest times to 1800. *By* G. F. HUDSON. *Arnold*, 1931. *pp.* 336 *and* 4 *maps*. 15*s*.

This is a wonderful book. In time the author ranges from Aristeas of Proconnesus to Lord Macartney's embassy ; in space right round the world both eastwards and westwards ; in subject-matter from the ultimate bases of politics and economics to the detail of Chippendale chairs. Bringing together facts from the most diverse sources, he focuses them so that thay make light instead of darkness. Most of his book is outside the purview of ANTIQUITY and yet one is led on through the ages, so that antiquity is seen to survive until the industrial revolution gave Europe the power to end it throughout the world, even ultimately in China. His great theme is that in European culture, owing to its Hellenic origin, trade has always been, more or less in different ages, necessary and respectable, whereas in the three other civilizations of China, India and Western Asia, founded on a land-revenue economy, it was essentially an extra, and the trader socially inferior not only to the governing class, but to the tiller of the soil. Hence in Europe commerce was a real power ready to make use of mechanical invention as a means not only of economic exploitation, but of political and cultural conquest.

For the early period it is interesting to note that Mr Hudson accepts the story of the northeastern or rather eastern trade-route in the sixth or seventh century B.C., as reported by Aristeas, preserved by Herodotus and interpreted by Tomaschek, and sees in his Hyperboreans the Chinese. After this first faint gleam of knowledge we are mostly occupied with the silk trade until the time of Justinian, and nowhere have I met so clear an account of it and of its economic and political effects. The journeys under the *Pax Tatarica* are scarcely our business, nor yet the way in which the desire for spices drew European explorers not only to round Africa, but to find America ; still less the rivalries of the XVIIth century or the wonderful achievements of the Jesuits.

But I cannot refrain from pointing out that Mr Hudson for the first time brings home to us how seriously our XVIIIth century was interested in China, so that its political ideas came near to being as deeply influenced by the Chinese model as was its art. It is curious that Mr Hudson has not quoted the romantic history of the flying gallop and its return to Europe, as set forth by S. Reinach. I have not noticed any other such omission in his book.

E. H. MINNS.

MANUEL D'ARCHEOLOGIE ORIENTALE. Tome III Histoire de l'Art (fin). Premier millenaire jusqu'à Alexandre. Appendices. *By* Dr G. CONTENAU. *Paris: Auguste Picard, 82 rue Bonaparte*, 1931. *Price not stated.*

With this volume Dr Contenau completes his handbook, and it preserves to the end the merits of all his work. A wide knowledge of the relevant literature, clear exposition and a sound critical judgment combine to render the *Manuel* indispensable. For the provinces with which it deals it replaces Perrot and Chipiez. Dr Contenau has made every effort to bring his work up to the date of publication, and includes discussions of the early painted pottery from various sites, of the objects from the 'royal tombs' at Ur, and of pottery and bronzes from sites in western Persia in the appendices.

It is always easy to find fault with handbooks, to point out that this or that has been neglected or inadequately treated, to select passages where the wording is erroneous or misleading. It is therefore not surprising that in dealing with so large a territory, and with such a mass of material, much of it new and more of it hardly discussed from the comparative point of view, Dr Contenau has exposed himself to criticism. Architecture for instance is but scantily treated. The discussion of the Nimrud ivories might lead the reader to suppose that one Nimrud hoard consisted of ivories that are exclusively foreign, not Assyrian, which is not the fact; and no mention is made of Professor Poulsen's acute distinction between the ivories of an Egyptianizing style which probably are Phoenician, and the totally different ivories which can be compared with the ivories from Ephesus and elsewhere. So who will may continue.

It is more useful, and more congenial, to point out the skill with which Dr Contenau has traced the comparable elements in the subjects and the treatment by Hittite, Phoenician, Assyrian and Persian artists. The world with which he deals, though extensive from the geographical point of view, was inhabited by men with a point of view as limited as a single nation's, in many respects. The *Manuel* will enable every reader to form an impression of the essential unity in the diversity of these national arts of the ancient Near East. Fascinating problems of course present themselves. Why did the passionate interest in animal forms common to the early periods at Susa and in Sumer pass to Assyria and Mitanni, during the second millennium, to find its most remarkable expression in the palaces of Nineveh and the bronzes of Persia and the Caspian area, whereas the Babylonians of the second millennium and later avoided animal designs on their seals, and only used stylized forms in architectural decoration? Has any critic of Hittite sculpture realized sufficiently that the formal *differentia* are due to the nature of volcanic stone? Will the truth, obvious from these volumes, that in the early Sumerian period sculptors and modellers experimented with high relief, and with the representation of the human form in action, only finally to refrain therefrom and found a tradition which imposed limitations that were voluntarily accepted, percolate into art histories? Above all, shall we ever be able to criticize and appreciate these monuments, *disjecta membra* at best,

since we can only grope after, and never fully understand, the ideas—mainly religious, of a time when religion was pseudo-philosophy, pseudo-science, in fact almost mental activity—which inspired them ? Are, that is, subject and style such safe archaeological guides as we incline to assume as certain, in this particular field, where so little was forgotten, so little learned? Dr Contenau, wisely, has not discussed such points, but we should all be thankful to him for providing text and illustrations, though the latter are not intended to be more than *pro memoria*, which enable us to dream about them.

SIDNEY SMITH.

BETH-PELET I. *By* FLINDERS PETRIE. *London : British School of Archaeology in Egypt and Bernard Quaritch*, 1930. *pp.* 26 *and* 72 *plates.* 50*s.*

Whether or no Sir Flinders Petrie is philologically justified in his identification of Tell Fara with the Beth-Pelet of the Old Testament, it is to be regretted that he has given his report this title. It was with an equal assurance that he once identified Tell el Hesy with Lachish, an equation which is now almost universally denied by archaeologists ; and it is at the least equally likely that his conjecture in the present case will be refuted by later discoveries. It is, in fact, a curious declension from that strict and even frigid scientific method with which this distinguished excavator has made us familiar, that he should have prefixed a conjecture—it is really nothing more—to the publication of such painstaking and detailed research. Even without the allure of a Biblical name and the advertisement of David's famous body-guard of Pelethites (once more the connexion is quite conjectural), Tell Fara has provided a mass of evidence well calculated to interest the student of Palestinian history. Situated in the Negeb some 18 miles south of Gaza, this fortress commanded the trade routes from north to south and from east to west. The actual site was an enormously strong one. On the east it over-hung the Wady Ghuzzeh lying a hundred feet below, while to north and south it was cut off by ravines from the surrounding plateau. Only on the west was it exposed to attack ; and here its Hyksos occupants defended it with a trench 80 ft. in width from edge to edge, a method of fortification peculiarly their own and strongly suggestive of the fortified '*tepe*' camps of Central Asia, their most probable homeland. That these ' Shepherd Kings ' were strangers from a far land has, of course, long been known, but the evidence of Tell Fara has added very considerably to our knowledge of them. It is now plain, as Sir Flinders Petrie observes, that they were not a whole population on 'trek' to a new home but a ruling caste accepting the culture of the countries in which they settled. In Egypt they became Egyptianized : in southern Palestine their utensils were those common to Syrian lands and only their scarabs reveal their connexion with the Delta. That this connexion followed hard on the XIIth Egyptian Dynasty (the first Hyksos Dynasty is the XVth) is now also established by the finds at Tell Fara, and the important conclusion is hence inevitable, that the XIIIth and XVth and XIVth and XVIth dynasties were actually contemporary with one another, and not, as has been assumed, in chronological succession. For the archaeologist a curious feature of this excavation was the entire absence of those black ' pricked ' vases regarded as so typically ' Hyksos ' in the dating of other sites. Sir Flinders Petrie attributes this to the closing of the sea-routes from the North, but the presence of these vases elsewhere in Palestine seems to demand some other explanation. Actually it is not impossible that further excavation of the Tell will disclose some specimens, and that their failure to appear hitherto has been a mere caprice of fortune.

Another important feature of the report is the new light which it casts upon the

Philistine occupation. It has been usual hitherto to regard these invaders from the Aegean as having arrived in Palestine at the beginning of the 12th century B.C. The discovery of five imposing Philistine tombs of which the earliest must be dated back to 1320 B.C. has completely upset this calculation and compelled us to revise our theories. The bearing of this new discovery upon the story of JUDGES has been well discussed by Prof. Garstang in his latest book. The use of pottery coffins with half-lids bearing a mask adds a very great interest to these finds. Sir Flinders Petrie quotes examples from the Delta, but it would have been still more apposite to mention those recovered from Beth Shean, where the garrison appears to have been of 'Aegean', if not of Philistine, origin.

In the later history of the site the evidence of the walls built by Shishak provides, perhaps, the most notable addition to our knowledge. The position of this monarch, in the excavator's words, ' has been greatly underrated. Not only did he conquer Jerusalem, but he was the greatest of builders in South Palestine. Tens of thousands of men must have been occupied in brick-making and building, for the great wall twenty-two feet thick, and the town buildings of Beth Pelet, also the entire rebuilding of Gerar '.

It will be apparent even from this brief *résumé* that Tell Fara has already yielded results of the greatest interest and significance. It is scarcely necessary to add that they are here presented to the student with that fulness, accuracy, and clearness which we have learned to expect from the publications of the British School of Archaeology in Egypt. The excellent air photograph of the site may, however, be given a special commendation.

W. J. PHYTHIAN-ADAMS.

ENGLISH COINS : from the seventh century to the present day. *By* G. C. BROOKE, Acting Deputy Keeper of Coins, British Museum. *Methuen*, 1932. *pp.* XII, 277 *and* 64 *collotype plates*. 22*s*. 6*d*.

Until a year ago there existed no up-to-date handbook on English coins to which the collector of moderate means or the reader who found himself interested in English numismatics, either from the historical or the artistic point of view, might refer. There was then no alternative but to seek for information in the publications of the Royal Numismatic Society, and the British Numismatic Society, or the Catalogue of English Coins in the British Museum (which extends no further than the Norman kings). Though these works must still remain the ultimate authorities for the study of English coinage it is no longer necessary to expend valuable time in searching through them for the detailed justification of an opinion when a summary statement of conclusions would suffice.

Mr Brooke has endeavoured to produce a book which would satisfy both the requirements of the general reader and the everyday needs of the collector, and he is to be congratulated on the degree in which he has succeeded. Each chapter contains a brief summary of the period in question followed by an enumeration and description of the different denominations and the principal varieties. These descriptions are models of lucidity and do not presuppose a knowledge of the literature of which the inaccessibility has been the occasion for the writing of the book. No attempt is made to reproduce the lettering of the inscriptions but allusion is made to the more important alterations, which can easily be traced in the plates. The inclusion of lists of moneyers of the various mints is very welcome.

Although important discoveries have been made in recent years with regard to 17th century numismatics, the chief interest of the book is inevitably centred in the treatment

of the earlier period. A warning is given against connecting the name *styca* (used to translate the widow's mite in the Lindisfarne Gospels) with the Northumbrian coins with which it has generally been associated, and the suggestion is made that the term is more likely to denote the debased late Roman or post-Roman *minimi* of which such numbers were discovered in the Lydney hoard.* Perhaps it would be kinder to believe that *styca* had no precise meaning than to regard the translator as envisaging the possibility of anyone expending valuable candle-power in the attempt to recover a coin of such infinitesimal dimensions.

It has long been realized that the written records of the Anglo-Saxons have in certain cases to be supplemented by evidence derived from coins, but the use of this source has led to many disputes. Thus Mr Brooke would like to fill the gap in the series of kings of Kent between the deposition of Eadberht in 798 and the accession of Cuthred in 801 with a king Eadwald whose pennies have hitherto been attributed to East Anglia, a kingdom to which a number of kings known only from coins have been attributed, often for no better reason than that the literary evidence for its history at this period is quite lacking. In the same way Beonna is taken from East Anglia and given to Northumbria (though lately a Mercian origin has been suggested). It is generally recognized that certain of the types (sometimes borrowed from the Romans) which were used by the later Anglo-Saxon kings have reference to contemporary events. Mr Brooke warns us that it is easy to over-elaborate this theme and perhaps it might have been better for this reason to abandon the supposed allusion to the coming of the Millennium found on the rare coins of Ethelred II bearing the Agnus Dei and the dove.

If the text provides much food for thought for the historian the excellent collotype plates are equally a feast for those interested in the artistic development of the coinage. The only complaint which may be made with regard to them is that the novice may find difficulties in distinguishing coins in different metals when illustrated on the same plate without the differentiating AU, AR and AE usual in numismatic works.

C. C. OMAN.

FIELD ARCHAEOLOGY : some notes for beginners issued by the Ordnance Survey. Professional Papers : New series, no. 13. *H. M. Stationery Office*, 1932. *pp.* 34. 6*d.* 1932.

ARCHAEOLOGISCHE KARTE DER RHEINPROVINZ : Ortskunde Trier-Mettendorf. *By* Dr JOSEF STEINHAUSEN. *Bonn : Peter Hanstein*, 1932. Published by the Gesellschaft für Rheinische Geschichtskunde as the third part of their Historical Atlas of the Rhineland. *pp.* 383, 38 *diagrams and* 32 *plates ; six maps in separate folder. Price not stated.*

It is being realized more and more clearly that archaeology consists in the study of the distribution of human cultures in space as well as in time, for as the mathematicians tell us with so much circumstance one cannot be said to exist without the other. This conception of the realm of archaeology as one of cultures in space-time accentuates the importance of mapping, both as an instrument of active research and as a most accurate method of record.

In this respect we in this country are fortunate, since it has been the traditional

* *See* R. E. M. Wheeler and T. V. Wheeler, *Report, Lydney Park* (Society of Antiquaries, 1932), p. 116 ff, and plates.

policy of the Ordnance Survey to indicate antiquities upon its published sheets. The implementing of this policy has been vastly accelerated and improved by the establishment of an Archaeology Officer. It will be realized, however, that in default of further official assistance the Archaeology Officer is forced to depend largely on the observations of the general public. The Professional Paper which we are noticing here is designed primarily for the education of such observers and the standardization of their map entries. As such it supersedes a publication of 1921. The improvements introduced in the new handbook more than justify the extra price. Apart from its more convenient format very useful bibliographies are given. Recent advances in knowledge are responsible for several additions or modifications in matter. Thus air-photography has been responsible for the inclusion of a new class of earthwork, the earthen and wooden circles, of which Woodhenge and the one near Norwich are recently discovered examples. Again a new class of camps has been added by the general recognition of the interrupted ditch-type, of which Windmill Hill and the inner camp at the Trundle, Goodwood, are classic instances. Pillow-mounds are now thought in many cases to have been no more than old rabbit warrens. Practical work in the field has caused the addition of an arbitrary division of stone circles into three classes or categories for the purposes of mapping. Finally we might add that, although designed primarily as an aid to the more accurate recording of our national antiquities, these notes are well worth having for private edification and especially for the addition of the bibliographies.

The second publication represents but a small part of an ambitious scheme for mapping the antiquities in the Rhineland. The exhaustive and meticulous character of the work may be gauged from the fact that the present part is concerned with an area of considerably under 500 square miles, occupying the southwest corner of the Eiffel country. Yet there are 383 closely packed pages of entries in the gazetteer arranged under placenames in alphabetical order. Loose finds, and sites excavated or unexcavated, are carefully noted together with explanatory diagrams and a full bibliography in the text. In a separate folder is a series of maps, indicating in different colours finds and sites of successive chronological divisions. As records these are admirable, though it is regrettable that the archaeological data had to be overprinted onto such unsatisfactory backgrounds. The method of layer-colouring has clarified our own maps so greatly that the simulated relief effect achieved by elaborate hachuring must be regarded as definitely archaic and out-moded. In this respect the archaeological mapping of the Ordnance Survey based on clean open maps with coloured relief is infinitely more satisfactory. The basis of the Roman Britain and other archaeological maps recently published by the Ordnance Survey is further enriched by the indication of the relevant surface-geology as reflected in natural vegetation. At the same time we in this country have still to contemplate the gazetteers which accompany the archaeological maps of the *Gesellschaft für Rheinische Geschichtskunde* with admiration and, if we are honest, with envy.

J. G. D. Clark.

ROMAN BRITAIN 1914-1928. *By* Sir George Macdonald. *British Academy Supplemental Papers no.* vi. *Humphrey Milford*, 1931. *pp.* 114 *with* 23 *text-figures and* 16 *plates.* *7s. 6d.*

The publication in German of Sir George Macdonald's survey of Romano-British archaeology marked an important epoch in post-war studies on Roman Britain. For it did more than present to the foreign student a chart of British archaeological progress:

it gave to the excavator at home an opportunity of learning what his fellows were doing and of seeing his own work in its proper perspective. For such reasons as these a demand arose that the work should be made more accessible to English readers, and Sir George Macdonald has met this demand by re-issuing it in English with important new matter as a British Academy Paper.

Thus the book takes its place as a successor to Haverfield's summaries of Roman Britain in 1913 and 1914. But as the author is treating of work done over a period of fifteen years, he has seen his way to give rather more space to the historical context of work done than the conditions of a yearly summary allowed Haverfield to do, and he has observations to offer on the general course of Romano-British history. These observations are as good as anything in the book : perhaps we may select the explanation of the Lancashire-Cumberland road system and the examination of the evidence for the abandonment of Britain as conspicuous examples of his lucid and suggestive treatment.

The qualities which distinguished Haverfield's work—sobriety and caution in hypotheses—are very noticeable here; and in dealing with a subject where conclusions are liable to continual modification, such caution is very much in place. The reviewer is bound to state that he read the book carefully to see whether any conclusion advanced had been refuted by subsequent discoveries, and he could not find one : it would appear that archaeology itself pays a tribute to the coolness of judgment and the freedom from unsure speculation which this book displays.

The material is handled by chapters which deal with the Legionary Fortresses, the Town Walls, the Auxiliary Forts in the various military theatres, the Saxon Shore, Civil Life in Town and Country, with some brief remarks on new epigraphic finds and on the general literature (where a well-deserved tribute is paid to Cheesman). The evidence from each site is concisely displayed and the chronological development indicated. The chapter on the Antonine Wall and Roman Scotland reaches, of course, a very high level, though the author is perhaps too modest over his own work at Mumrills, the report of which must remain for many years the standard authority on Roman bathing establishments. We note that the author clings firmly to his view of a Roman occupation of Scotland extending to Trajan's reign and is unmoved by the doubts which Mr Birley has expressed.

The chapter upon the Towns shows clearly the two waves of urbanization under Agricola and in the early second century : and here an admirable summary of the excavations at Wroxeter whets our appetite for Professor Atkinson's long-promised report. A date of *c.* 65 is accepted for the walls of London and Colchester, and the analogies between the Balkerne Gate of Colchester and various first-century town gates is stressed ; but a plan of the Balkerne Gate might well have been included, as it is not only 'unique in Britain', but in some of its details unique aparently in the whole Empire. An informative section of the Caerwent town-wall makes us hope for a re-examination of the Silchester walls, which present several similar features.

A brief chapter describes work done in the country districts (Villas, Temples, Potteries) ; and here a criticism must be entered. The country life of the Province is allowed only 12 pages as against 73 devoted to military works ; and of these twelve villas have only two pages, villages none at all. This may suggest, expecially to a foreign reader, either that the Countryside in Roman Britain was unimportant, or that experts are not alive to its importance; and neither of these implications is true. The interesting remarks on Traprain make one regret that no similar treatment is given to a South of England native site such as Ham Hill or Park Brow. The distinction between

Celtic and Saxon fields is emphasized by the author: but it should be remembered that Mr Crawford's aerial work was confined to the area of the native settlements, and that in regard to the origin, development and end of the villa-system we are almost as much in the dark as our predecessors.

But it is ungenerous of a reviewer to carp at the omission of what particularly interests him : the book is full of good things and not least valuable are the full bibliographical references which it supplies.

Finally, a tribute must be paid to the publisher. Type, make-up, and illustrations are alike excellent : and how a book of this kind can be published for no more than seven shillings and six pence, it is difficult to imagine. C. E. Stevens.

THE FIGHT FOR AN EMPIRE : a translation of the third book of the Histories of Tacitus. *By* W. J. Woodhouse. *Sydney*, 1931. *London : Australian Book Co.*, 16 *Farringdon Avenue, E.C.* 4. 1932. 6*s*.

Professor Woodhouse has gone far to success in the hard task he has set himself. This is how he translates : ' Hardly had Martialis got back to the Capitol, when the maddened soldiery was on them. Commander-in-chief was none ; each man took orders from himself alone. Swiftly through Forum swept the mass of men, past the temples rising above it ; then breast the hill in front, right up to the outermost gate of the Capitoline fortress'. It is clear what he wants: the terseness, the sharpness, the biting edge of the sentence ; the avoidance of verbosity on one hand, commonplace on the other. The danger is that choice, unprosy diction may decline to Wardour-Street ; that happens here and there, and especially—an orgie of it—in the summary or ' Brief ' prefixed to the tale. But it is a fine translation : take any chapter and try to better it, and you will find it hard. Introduction and notes are excellent—short and full of matter, like the text itself. R. G. Collingwood.

THE TRANSITION FROM ROMAN BRITAIN TO CHRISTIAN ENGLAND (A.D. 368–664). *By* Gilbert Sheldon. *Macmillan*, 1932. *pp.* xxiii, 219, *with map*. 10*s*.

This volume is the last memorial of a man whose memory deserves to be kept green : a man crippled from babyhood with infantile paralysis, unschooled and uncollegged, but living for over sixty years an active and happy life of travel and study, a lover of books and a lover of the English country. In prefatory notes to this work—which he completed only a few months before his death—his sister, Miss Lilian Sheldon, and Mr Walter de la Mare have given us a charming picture of his life and personality, a picture confirmed by the pages of the book itself.

Such a book, written in such conditions, is not easy to review. One would rather extend the good rule, *de mortuis nil nisi bonum*, from the writer to his book, and be content with a testimony, a sincere one, to its merits. But, after all, the book is not dead : and it is a poor tribute to an author's memory if one refuses his work the honour of being candid about it. First, then, it is the work of an amateur in both senses : a labour of love, done with zest and enthusiasm, but also done without the severe discipline of the professional. The author knows his subject chiefly from more or less recently published books, and even of these there are some, of first importance, which he does not know. To take one instance only, he has much to say of the Romanization of Britain, but does not quote, and seems not to have read, Haverfield's classical essay on that theme. The

periodical literature he hardly touches : for instance, he quotes only two articles from *Archaeologia*, and none from the *Journal of Roman Studies*. This is not because, knowing the material, he refrains from a display of bibliographical window-dressing ; one who had read widely would not assert that the Antonine Wall was reconstructed by Severus, and would know that the book from which the assertion was taken is not trustworthy ; nor would he turn the well-known Irish writer Tighernach into ' Tigernach ', one of the ' Welsh annalists '. In a word, the book is not a learned book, and must be judged on merits of a different kind.

Secondly—not, perhaps, rigorously distinguishable from the first point—the logic of the argument is often impaired by the vice of special pleading. The whole book is a plea for extensive Romano-British survivals in Anglo-Saxon Britain: and the division between the ecclesiastical provinces of Canterbury (originally London) and York is explained as based on ' the old-time division of Britain under the Empire into a southern and a northern province '. What division is meant ? The Severan divisions of Britannia superior and inferior were quite different, and in any case were superseded by the still more different fourth-century sub-divisions recorded in the *Notitia* ; the military spheres of the Dux Britanniarum and the Comes Litoris Saxonici were in no sense provinces. It is difficult to attach any definite meaning to such words. Again, the author is trying to minimize the extent of the early Anglo-Saxon kingdoms and settlements, and argues that the victories of Ethelfrith cannot have extended his dominions west of the Pennines because Loidis and Elmet remained independent. But Loidis and Elmet were in the south ; how does this prove that there was no advance in the north ? And, even in the south, how does it prove that Loidis and Elmet, for their few remaining years of independence, were not *enclaves* in Northumbrian territory ? Are we to reject the story of the foundation of Rhodesia because the Boer States on the Vaal and Orange rivers remained independent? Again, the writer wishes to prove the survival of Romano-Celtic civilization on the borders of Wales : for this, he says, there is ' strong archaeological evidence ', and this evidence turns out to consist of hanging-bowls, which, through hastily reading a sentence in Baldwin Brown, he imagines especially common in that region. Again, he wants to prove the widespread continuance of a Roman tradition of mason's work. Having argued for such a survival at Canterbury, he argues for another at York, the evidence being that a stone church was built there under Paulinus's direction, and ' nothing is said of the importation of workmen '. When Ethelberga and all her *entourage* (including Paulinus himself) came from Canterbury, could any *argumentum e silentio* be weaker ?

But if we cannot look either for sound scholarship or cogent reasoning, it does not follow that the book is devoid of value. Mr de la Mare well refers to Mr Sheldon's earlier historical essay (*From Trackway to Turnpike*) as a ' romance '. An historical romance need not have deep learning, and it need not have subtle logic, but it must have intuition ; and that is Mr Sheldon's great gift. He has read enough about his subject to see that the old view, according to which Anglo-Saxon England owed everything to its Teutonic immigrants and nothing to its Romano-Celtic predecessors, is damaged beyond repair. He sees that the work now being done on the subject consists, in the main, of an attempt to throw bridges here and there across the gulf which used to divide Roman Britain from Anglo-Saxon England. The idea that there is, after all, a genuine continuity between these two, has seized upon his mind, and he has vividly imagined a picture of the one changing, gradually and almost imperceptibly, into the other. Snatching at any straw of evidence that would strengthen his picture, he persuades himself that

the supposed gulf is just an illusion. This is not so ; the thesis of the book is hopelessly one-sided, an exaggeration based on facts unfairly selected and evidence unduly strained. Yet it is all to the good that such an exaggeration has been put forward. Instead of starting from the old ' clean-sweep ' view and asking where and why we must depart from that, we can now start, alternatively, from the opposite end : we can take Mr Sheldon's thesis and ask where and why we must refuse to accompany him. For serious students, that is an advantage.

R. G. COLLINGWOOD.

CHRISTIAN ART IN ANCIENT IRELAND : selected objects illustrated and described. Edited on behalf of the Irish Free State by ADOLF MAHR. *Dublin : Government Publications Sale Office*, 1932. Vol. I, *pp.* XXVII *and* 80 *plates*. 21*s*.

This album may fairly claim to be the most important publication of 'native' archaeological material that has ever been issued by a national museum in these islands. That, of course, sounds a ridiculous and hyperbolical thing to say, but it happens to be true. I know only too well the need in all departments of our archaeology for adequately illustrated catalogues of domestic antiquities, and I greet this volume with hearty congratulations. It is, indeed, very much to the credit of the Free State Government that a great opportunity has not been missed. It would have been only too easy to issue, as a sufficient compliment to the Eucharistic Congress, a picture-book containing a few illustrations of notable objects; but the Royal Irish Academy and the Editor, Dr Mahr, have seen to it that the funds available were used for the production of a book of much more than 'souvenir' value. It is true that we are modestly asked to regard it as merely an attempt towards a survey of Irish metalwork (together with a few plates illustrating MSS and ecclesiastical architecture), but the attempt is obviously going to be so successful that I shall brush this disclaimer aside and declare roundly that the Editor has collected an invaluable and comprehensive *corpus* of material that will be a permanent delight to the student. What less can be said of a book that illustrates sumptuously and for the first time the rich store of Irish material in the Scandinavian museums side by side with the metalwork from these islands ? The album is, in truth, an extraordinary achievement, for the Editor and his Committee tell us that they had only five months in which to prepare it; and all English readers will readily endorse Dr Mahr's tribute to the continental museum-officials who responded so quickly and so effectively to his appeal for help.

The great thing was to seize at once and to make the most of a chance that might not be repeated. It is a small thing that the work appears with some blemishes that are the result of its over-hasty production, and we need not boggle at a plate which is upside down or at a few mis-spellings in the legends. More serious, however, is the effect of this haste upon the plan of the book, which has real, though unavoidable, disadvantages. Thus the work is to appear in two parts, and while the first volume, now published, contains 80 out of the 130 plates, the descriptions and bibliographical details for these 80 are kept back for the second volume. In the meantime we have only the Editor's all too short ' Introduction ' as an aid to the study of the illustrations. Dr Mahr describes briefly what he considers to be the four main styles in early Irish art (Vernacular Keltic or ' Scotic ', Hiberno-Viking, Last Animal Style, and Hiberno-Romanesque). I think that his dates and distinctions are in the main sound, though the Vernacular Style is really something more than the characteristic of a mere phase, and it most certainly deserved fuller treatment, in particular as regards the origins of the favourite ornaments.

Above all, I feel that Dr Mahr might have clarified the position had he made some reference to the problem of the Lindisfarne Gospels. But I expect he thinks, as I do, that it is very dangerous for any of us to write about Keltic art before we are in possession of Mlle. Henry's long-awaited book. Nevertheless he has made one very good point in advance by emphasizing the probability that Gaelic expansion may be responsible for the post-Roman revival of Keltic art, and he does well to remind us that the total loss of the output of the 'perishable' crafts (woodwork, textiles, leatherwork, basketry) must be taken into account, not only in Ireland but also in Britain.

T. D. KENDRICK.

A HISTORY OF SURNAMES OF THE BRITISH ISLES. *By* C. L'ESTRANGE EWEN. *Kegan Paul, Trench, Trubner and Co.*, 1931. *pp.* xx, 508. 25*s*.

This is the fullest attempt which has yet been made to deal with the surnames of the British Isles on a comprehensive scale. To judge from the copious bibliography at the end of the volume the author has consulted most of the writings on the subject, of all periods and of all countries. He has also been through a great deal of material both printed and manuscript from Anglo-Saxon times to the present day, and this has enabled him in most cases to trace with success the history of the names with which he deals.

The author begins with a long discourse on the history and origin of languages and names, with a discussion of the different races by which this country has been peopled during historical times. Beginning with the Old English period, he deals in successive chapters with documents illustrating the gradual development of the surname from the 10th to the 17th century. The number of personal names said to survive from Anglo-Saxon times is probably not so large as the author would have us believe. Wolsey may derive from a place-name rather than from the OE personal name Wulfsige.

Mr Ewen is most at home with names of Romance or Germanic origin, but he has also attempted, in many cases with success, the interpretation of Celtic surnames of Welsh, Gaelic or Manx origin. His treatment of Cornish names is much less satisfactory; he is evidently not aware that hardly anything that has been written on this subject is of any value, and that much is entirely worthless. Many of his supposed Cornish names can easily be explained from other sources, *e.g.* Bullivant, Forder, Henty, Kimber, Quick. Pennycomequick, a place-name found in Devon and elsewhere, is no doubt a nickname for a prosperous farm. It cannot in any case be for 'head of the combe creek', for the Cornish definite article is *an*, not *y*. Occasionally his transcriptions of names in early documents have led him to wrong conclusions. He cites the surname *atte Boscall* from a Devon document as a proof of quite late Cornish influence in that county. But this surname, which occurs under Axmouth in East Devon, is clearly to be transcribed *Bostall*, the man in question taking his name from Boshill in Axmouth, earlier *Bostall*, *Bosthill*. Similarly the Devon surnames *Bydene*, *Shute*, *Trewen*, *Lantshar*, *Velye*, found in 1332, can all be traced to place-names of certain English origin in the county.

On pp. 238–40 a list is given of local names peculiar to the different English counties. In some cases, however, we may doubt whether the place in question gave rise to the surname. Cornhill, a London City street-name, can hardly have given rise to a surname such as Cornell. There are many real place-names Cornhill in the country. Rodney (Somerset) is not a place-name, for the village was earlier Stoke Rodney, the manorial addition being the name of an early owner.

The statistics which are given here and there to illustrate the various name-types are interesting. It will be seen that the proportions vary greatly in the different localities. Surnames of local origin are most frequent in the north and in the southwest and are practically absent in Wales and Ireland. In the former country the genealogical type is practically the only one represented alike in modern and in medieval times. This is rather strange in view of the fact that Cornish and Breton surnames are largely drawn from local sources.

The author deals at some length with the surname Shakespeare in his endeavour to prove that the name is not, as popularly supposed, to be interpreted literally. The etymology is perhaps best left open, but one is inclined to accept the obvious meaning of names like *Hatecrist*, *Shunecrist*, found in early documents, for which Mr Ewen would seek a different interpretation.

A large number of quaint surnames found in early sources do not seem to have survived. As examples may be noted *Cockesbrayn* (1275 Sussex), *Doggetail* (1327 Worcester), *Evilchild* (1275 Cambridge), *Lickefinger* (1205 Norfolk), *Rotenhering* (1331 Yorks), *Drinkedregges* (Lancs), *Strokelady* (1377 London), *Spurnewater* (1274 Norfolk). In a few cases where the meaning of the offensive, or at least contemptuous, nickname has been forgotten, the names have survived, often in a corrupt or unrecognizable form.

The book should prove of some use to those who are interested in the subject, though the number of well-known surnames which have been omitted is disappointingly large. A short chapter dealing with all, or at least some, of the best known names in literature, history and politics would have been a welcome addition to the book and would have helped to satisfy the interests of the general reader. J. E. Gover.

LA DEUXIEME CAMPAGNE DE FOUILLES A RAS-SHAMRA. (Printemps, 1930). Rapport et études préliminaires. Extrait de la Revue Syria, 1931. *By* F-A. Claude Schaeffer, Charles Virolleaud *and* Francois Thureau-Dangin. *Paris : Paul Geuthner*, 1931. *Price not stated.*

The excavations which opened at Minet-el-Beida and Ras Shamra in 1929 were almost at once attended with startling success. In this small but fertile territory some ten miles north of Latakia were established in the second and first millennia B.C., the principality and city of Sapuna—the Biblical ' Zephon ' of Exodus, xiv, 2. Here, in comparative remoteness from the great highways which conquering armies followed, there sprang up a library and a college, where the scribes of the 13th century might learn their erudite profession. The evidences of their labours came rapidly to light in the first season's exploration, when tablets were found which revealed the existence of a Canaanite literature closely akin to the Hebrew. The present volume records the results of the ensuing year's expedition which not only confirmed the preliminary surmises of the excavator but added to the list of languages studied at this Academy (Accadian and Sumerian vocabularies had already been found) a fourth and entirely unexpected stranger, related, as M. Thureau-Dangin shows reason to believe, to the speech of the Mitanni or Khurri of northern Mesopotamia. Of the Canaanite tablets already mentioned, M. Virolleaud publishes here (with cuneiform facsimiles) a number of fragments containing an epic poem of the Phoenician divinities El, Asherat, Anat, Mōt, and Aleïn (the last-named hitherto unknown). Side by side with these, as it were, were found a typically Egyptian shrine dedicated to Baal Sapuna (Baal-Zephon) with a basalt stela suitably inscribed with Egyptian hieroglyphs, and, close to this, a smaller

and much ruder monument, in which the god combines the attributes of an Egyptian and a Hittite deity! The confusion of races, languages and cultures seems, in fact, to have been unusually involved in this small maritime State, and the presence of Mycenean vase-fragments may prepare us for even more striking revelations. For unless all the evidence deceives us, Sapuna must have been very closely connected with those mysterious Aegean Keftiu whom the XVIIIth Egyptian Dynasty knew so well. M. Schaeffer does, indeed, report a lower stratum of occupation, the vases of which, as his excellent photographs testify, must belong to the latter half of the Middle Bronze Age. Between this and the 13th century library and temple there is still a gap whose history remains unknown. Ras Shamra has certainly much more to tell us. W. J. PHYTHIAN-ADAMS.

MIDDLESEX IN BRITISH, ROMAN AND SAXON TIMES. *By* SIR MONTAGU SHARPE, K.C. *Methuen*, 1932. *pp.* XIX, 240, *with* 12 *illus.*, 9 *maps and plans.* 15*s.*

It is to be assumed that publishers, when they write commendatory puffs of their own productions, do not wish to be taken too seriously by the serious. Otherwise it is a little astonishing that Messrs Methuen, who are responsible for quite a number of good archaeological books, have seen fit to describe this volume as a ' valuable work which appeals to the archaeologist, the historian and the general reader '. Now, although the archaeologist and the historian are capable of looking after themselves, the general reader is only too willing to put his trust in the expert, and he should therefore be treated with sympathy and with consideration. He ought to be put on his guard against mere theory cleverly or convincingly presented as indisputable truth. He ought not to be told, for example, that ' the earliest inhabitants [of Middlesex] of whom anything is known were the Neolithic '. He ought to understand that a distinction is usually made between the cultures of the Neolithic and Palaeolithic periods, and he should realize that the term ' pre-Celtic ' as applied to the peoples and industries of the Stone Age has little more meaning than pre-Cockney.

The best passages in this book are those which are frankly imaginary. He would be a peevish reader who was not thrilled by the distinguished person from Verulamium, ' of noble mien with fair moustache and long hair turning grey, now stepping out of the esseda (war chariot) '. Nor can we read unmoved the words which paint so vividly the picture of Lady Helena : ' This golden-haired British princess, with lovely face and delicate complexion, is both as kind and good as she is fair, and is adored by young and old throughout the tribe. Fine of stature, she sits her horse with inborn grace—' and so forth. Whatever may be the archaeological value of such passages, they certainly do show us how to brighten the style of works that are often so dull and heavy and so doggedly devoted to mere fact.

But we are not prepared to follow Sir Montagu when he talks about Caesar's passage of the Thames, or the Roman land-system, or the warfare of the Saxon invaders. Caesar is assumed to have crossed the river at Brentford, and the most convincing proof of this is a granite monument unveiled by the late Duke of Northumberland in 1909. As for the muddy stakes which are drawn out of the river and used as props for ingenious theory, it is quite obvious that no one can determine their date or purpose ; many of them are almost certainly the remains of eighteenth-century fishing-weirs. Nor does the argument in favour of a neat rectangular division of Middlesex by Roman surveyors rest upon evidence of a more substantial kind. Ingenuity is not enough ; there must be proof, and the proof is lacking. There is no evidence to show that Middlesex churches are

built on the sites of Romano–British chapels ; no evidence to show that tumuli were used as surveyor's marks or boundaries by the Romans ; no evidence of 'the general continuance through the Saxon era of the paginal divisions '. There is, on the contrary, plenty of evidence (though not in this book) to show that the Roman occupation of Middlesex was extremely scattered, never far from the vicinity of the main highways or the rivers, and in no case (except at Brockley Hill) of any considerable local extent. It could not well have been otherwise, for the greater part of Romano–British Middlesex was uninhabitable. Sir Montagu is not the first man to be fascinated by the delightful pastime of ruling lines on Ordnance maps, and with no small ingenuity making up a pattern out of churches, trees, bushes, mounds, occasional stones or simple tradition.

The book is well bound and has an excellent frontispiece. C.E.V.

THE CIVILIZATIONS OF THE EAST : THE NEAR AND MIDDLE EAST. *By* René Grousset. *Hamish Hamilton*, 1931. *pp.* 404, *and* 313 *illus.* 25*s.*

The first volume of M. Grousset, Conservator of the Guimet Museum, covers the civilization of Egypt, Babylonia, Assyria, ancient Persia and of the Musulmans both in Mesopotamia and Persia. An introduction describes the primitive civilizations (neolithic and early metal ages) in Egypt and nearer Asia. In this chapter, which takes into account the latest discoveries in these various regions, the author provides a summary which could not have been given at all a few years ago. There is even a hint of the connexions which may have existed between these remote cultures and primitive China. But while M. Grousset has attempted to indicate the main outlines of the civilization of each region and period, it is the history of art which has pride of place in his narrative. Numerous illustrations representing the chief monuments of each period, whether still *in situ* or scattered in museums, assist the reader to follow the artistic development described in the text.

A journey of the author's into Persia has enabled him to evaluate the art of the Safawid epoch, and he gives illustrations of several monuments in Ispahan belonging to this period ; nor has he overlooked miniatures in manuscripts.

The book is the first of a series designed to cover the Far East as well ; it is clear and well-arranged, and will be of great use to travellers, and forms an excellent introductory handbook for those who wish to study the art of the East. G. Contenau.

THE PRISMS OF ESARHADDON AND ASHURBANIPAL, FOUND AT NINEVEH, 1927–8. *By* R. Campbell Thompson. *The British Museum*, 1931. *pp.* 36 *and* 18 *plates.* 10*s.*

The excavation of Nineveh, which is the source of most of the Assyrian collections at the British Museum, has been resumed by Dr Campbell Thompson. Amongst the results of the campaign of 1927–8—historically one of the most important—is the discovery of a six-faced prism some distance from Nineveh at the place where Sennacherib had built a palace for his son. The prism, which is dated 673–2 B.C., recounts the history of Esarhaddon. This document is important both because it is more complete than any of those which we have hitherto obtained and because it alludes to the assassination of Sennacherib by his sons. The vagueness of the text and its obscurity are evidence of the embarrassment of the scribe who had to gloss over an incident well known to his contemporaries living at the court of the king. In addition to the transcription and translation of the text Dr Campbell Thompson gives us a reproduction of it in impeccable

script. Another prism, summarizing the list of Assurbanipal's buildings, concludes the volume ; it was found under the southeast gate of the Temple of Nabu at Nineveh. These two documents will be welcomed by historians as well as by Assyriologists.

G. CONTENAU.

PHOTOGRAPHS OF CASTS OF PERSIAN SCULPTURES OF THE ACHAEMENID PERIOD, MOSTLY FROM PERSEPOLIS ; 12 *plates. The British Museum*, 1932. *Cased copy*, 7*s* ; *set of plates loose*, 6*s* ; *separate plates*, 6*d each.*

The ruins of Persepolis form the most important architectural and sculptural group of the Achaemenid period. At Susa the remains of this period are confined to a single phase, the reign of Artaxerxes Mnemon (404–359 B.C.) ; whereas at Persepolis the developments of this style can be studied, the monuments ranging from the reign of Darius to Artaxerxes Ochus (522 to 338 B.C.). Persepolis lies in the south of Persia, not far from Shiraz ; and by reason of its remoteness the ruins have survived down to the present day. This inaccessibility however was double-edged ; while it preserved the ruins from destruction it also kept them hidden from the eyes of the world, and few travellers went there. Several expeditions went to obtain casts of the principal monuments, notably those of Lottin de Laval in 1884 and Cecil Harcourt Smith in 1887. A set of these casts was exhibited last year at the British Museum, for in default of the inaccessible originals they became documents of the first order of importance ; and the British Museum authorities have now published reproductions of some. The first two plates represent the king (probably Xerxes) on his throne, attended by small figures symbolizing the different peoples in the empire. Several plates figure vassals offering presents to the king. Here and there the artist, by adding a tree, suggests the plantations with which the northern part of the terrace of Persepolis was adorned ; the vassals pass through them to approach the king. This scene is carved on the wall of a staircase built by Artaxerxes III. Other panels have symbolic scenes, such as a lion attacking a bull, a motif dear to Mesopotamian artists right back to the dawn of history. Others again represent the king killing a griffin, symbolizing the struggle of good and evil. One plate has been taken from the monuments of Pasargades ; it represents, not, as was once thought, Cyrus, but a winged spirit derived from the Assyrian conception of supernatural beings.

G. CONTENAU.

MESOPOTAMIAN ORIGINS ; the basis population of the Near East. *By* EPHRAIM A. SPEISER. *Philadelphia : University of Pennsylvania Press ; [London : Milford]*, 1930. *pp.* 198. 12*s* 6*d.*

The first excavations in Mesopotamia (at Khorsabad and Nineveh) revealed the civilization of Assyria. A little later the discoveries of de Sarzec at Tello made us acquainted with the civilization of Sumer. The original conception of a Mesopotamian civilization founded by Semitic Assyrians was abandoned and replaced by the conception of a civilization originated by non-Semitic Sumerians and adopted at an early date by Semites. The discoveries of recent years have set the problem a stage further back and have obliged us to recognize the existence, before the Sumerian period and throughout Western Asia, of a period characterized by the use of painted pottery—which vanished completely when the Sumerians came upon the scene and which cannot therefore be attributed to them. Simultaneously there was recognized over the same region a

conglomeration of peoples whose languages had sufficient points of affinity for them to be grouped together—languages, however, which are represented only by the persistence of their proper names. These peoples, first studied in Asia Minor, have been called Asianic. Their range, traced in Mesopotamia as far as the Zagros range and Iran, has caused Professor Marr to give them the name ' Japhetic ', which would thus be applied to those peoples of Nearer Asia who were neither Semites nor Indo-Europeans (at any rate upon their first emergence).

Now Professor Speiser has taken up the problem again. After a general introductory chapter he deals successively, from the archaeological and linguistic standpoints, with the different peoples which can be grouped together under this heading ; with the connexions between Sumer and Elam (the name given to ancient Persia) ; between the Lullu and the Guti inhabiting the foothills of the Zagros range ; and between the Kassites and the Hurri. Many of these peoples have long been known. The connecting link is established by the knowledge we have recently obtained about the Hurri, who lived in Northern Mesopotamia and enable us to connect the peoples of Asia Minor with those of Zagros and Iran.

In the course of these novel researches the author has come to certain conclusions of a detailed character whose validity will probably not be maintained in their entirety in the course of the future ; but the foundations appear to me to be solid ; they consist in the presence of an indigenous population with marked affinities, spread over Asia Minor and the Iran plateau before the arrival of the Sumerians. We cannot commend too strongly this book of Professor Speiser's. G. CONTENAU.

THE LAND OF TROY AND TARSUS. *By* J. E. WETHERALL. *Religious Tract Society. pp.* 264, 16 *plates. 7s 6d.*

A vast and almost bewildering number of subjects is treated in Mr Wetherall's book, as he begins with the Hittites, 1500 B.C., works through Greek and Roman history to New Testament times, then deals with the medieval period and the Crusaders, and finally brings it up to date by chapters on ' The Coming of the Turks ' and ' Angora ', concluding with the declaration of the Turkish republic in 1923, giving two portraits of Mustapha Kemal Pasha.

With such a wide range as this it is impossible that the book should entirely lack interest, but the methods adopted are unfortunate and apt to lead to confusion. A complete disregard of sources and evidence, and an elementary treatment of the subject matter, such as is only justifiable in a book intended for readers who have no knowledge of the subject, is coupled with the assumption of a thorough grounding in ancient history and the consequent omission of important facts—which leads to a very distorted view.

For instance, we have only a most abrupt introduction to Julius Caesar :—' After Julius Caesar had completed nearly a year of inglorious life in Egypt, in petty battles and dalliance with Cleopatra, he set sail for Syria '—no mention of his previous engagements or his campaigns in Gaul.

The method is at fault here, in that it gives chief prominence to places, and regards events merely as important when they occur in those places. This inevitably detracts from their importance, as so often the most interesting episodes take place elsewhere. Thus we hear of Cyrus' march from Sardis to the Syrian Gates, and again of the return of the Ten Thousand from Trapezus to Byzantium, but the intervening events and the battle of Cunaxa are slurred over, and we are given the cold comfort that ' they are

all recorded fully and vividly in the pages of Xenophon '. Other chapters, such as 28 : ' The Cilician Gates ', consist of a string of events in chronological order with no logical connexions save that of their common locality, which also tends to become tedious.

Throughout, the book is characterized by a sweeping lack of discrimination and a total disregard of the difference between fact and fable. Thus Mr Wetherall does not scruple to tell us that Solon had an interview with Croesus, a fact which even Plutarch admits ' some think not agreeable with chronology '. We are also treated to a complete biography of Homer ; of how ' when still a lad ' he was taught by Phemius, then travelled abroad, and established a school in his old age at Chios, where he ' acquired a considerable fortune, married, and had two daughters '. We also read with interest that the Seven Wise Men were elected in 652 B.C., and that the Great Mother of the Gods was called ' Ma '.

On the whole the production is good, and we have only detected two misprints (pages 72 and 149) ; but the selection of plates has been rather unfortunate ; the map at the beginning is far too cramped and confused to be of any value, while the portraits are all badly reproduced and some, notably those of Caesar and Cleopatra, seem to be the ugliest available.

However there are undeniably some good stories in the book, told in an easy, vivid style, while some happy phrases such as ' Eastward Ho ' have been coined to form effective chapter-headings. In short, if not taken too literally, it proves pleasant light reading.

ELEANOR DOBSON.

THE THEORY OF SPEECH AND LANGUAGE. *By* ALAN H. GARDINER. *Oxford : Clarendon Press*, 1932. *pp.* x, 332. 10*s* 6*d*.

Dr Gardiner's book, which as he tells us, has been pieced together gradually in his spare moments during the last four years, is a serious attempt to arrive at the philosophic principles governing one of the most characteristic of human activities.

The book appears at a time when a critical interest in language study is developing apace. For this there are several obvious reasons. Of primary importance is the tremendous increase in the number of new literary languages. The opening up of new territories, particularly in Africa (one consequence of the triumphant development of the motor-car) has brought to the fore the problem of educating hitherto unlettered primitive tribes. African education is very rightly based on first teaching the child, whenever possible, through the medium of its mother-tongue. Scores of African languages can now boast an alphabet and even the beginnings of a printed literature. Again the Soviet Union, standing as it does for the principle of a voluntary alliance of free peoples, presupposes the provision of equal opportunity for all to receive an adequate education, irrespective of nationality. Here too a considerable number of languages have been reduced to writing for the first time. Indeed no less than 72 out of the 140 languages spoken in the Union are reported to have a printed periodical literature.

Another factor which has been stimulating the study of language theory is what may be termed the ' rationalization-motive '. The stress of competition in the modern world operates continuously to suggest the advisability, in any and every way, of saving superfluous energy. In such circumstances the schoolmaster, faced with heavy demands for allocation of time to new subjects, like others, succumbs to the rationalization urge and begins to cast longing eyes on periods sacred from time immemorial to the study of grammar. His sub-conscious self doubtless abets him by suggesting that the removal of this item from the curriculum would receive the hearty support of his pupils.

' It is in periods of transition like the present ' writes Dr Gardiner, ' that the never-ending struggle between authority on the one hand and the spirit of reform on the other, becomes most insistent and vocal '. It is but natural that the conflicts ' familiar in the contemporary world of politics ' should repeat themselves ' even in so apparently tranquil a backwater as that of grammatical lore '. We do not, for reasons which will be stated, believe that the work under review has once and for all established the science of linguistics upon an unshakable pedestal, but it is no over-statement of its merits to suggest that it is a book which should be most carefully read by all serious students. Its pages, written in a style as subtle as it is clear, are essentially thought-provoking, and many are the instances where a new and vivid light is thrown upon ancient conventions.

The analysis of such conventional terms as ' speech ', ' language ', ' sentence ', ' clause ', ' noun ' reveal depths of significance which are certainly overlooked by ordinary people, and have rarely, if ever, been apprehended so precisely even by professional grammarians. For instance, Dr Gardiner makes a valuable contribution to linguistics in advancing his conception of the act of speech as being in the nature of a quadrilateral affair comprising the four factors : the speaker, the listener, the words, and the things (spoken of). The actions and reactions of the several factors on each other are most carefully thought out and presented in chapter II, suggestive picture-sequences and diagrams being utilized for the purpose.

It is when the author reaches his third chapter and deals with the historical significance of language (and therefore by implication, of grammar) that he comes nearest to a satisfactory evaluation of the place of linguistics in philosophy. We could wish that he had expanded sections 34–39 to form the main part of the book. For when we would attempt to understand why it really is that we bother ourselves with the study of grammar and linguistics, we are forced back, sooner or later, to a consideration of the functions of speech and language as they manifest themselves today, and even then cannot get far until we begin to study their history. That approach takes us, as Dr Gardiner has realized, out of the realms not only of linguistics, but even of sociology, right into biology.

Just as soon as, in the evolutionary trend, organisms not permanently joined together came into existence, the need arose for them to establish temporary contacts ; at first of course, only for the purpose of procreation. As life became more complex species with a definite degree of social life established themselves. Amongst terrestrial animals, including birds, we find the utilization of the specialized organs of alimentation for the purpose of sound-emission widely developed, and, as a corollary, the evolution of the organ of hearing. The extent to which the social behaviour of various species is dependent upon inter-communication by sound undoubtedly varies. It is interesting to note in this connexion that recent studies of the chimpanzee have established the fact that it has at its command at least nine different sound-series, each with a special significance.

Man, so far as is known, has gone further than any other animal in his use of sound as a means of inter-communication, and is perhaps alone in having evolved *variable* sound-patterns which can properly be called ' language '. But the most significant fact to remember is that his use of all this has varied at different times and in different circumstances, and in all probability will continue to do so in the future.

Dr Gardiner's contention that language arose through concentration on speech as the most serviceable means of inter-communication between human beings will be generally accepted. Human vocabularies have evolved through this process of

mouth-to-ear communication. But, at varying dates after Man had acquired the faculty of speech, with its armoury of sounds (words) shared between individual and individual, he has been able to transfer his word-technique (or language) to other vehicles than speech. Perhaps the greatest achievement amongst these transferences was when he invented the art of writing, and thus was enabled to inter-communicate by a hand-to-eye process which overcame limitations of time and space. And he did not stop at that. For the assistance of such of his fellows as were afflicted with the calamity of blindness, he extended the use of language to a hand-to-hand business (braille). After a long period during which the hand-to-eye means of communication has been steadily advancing in importance, we are now confronted with fresh possibilities for the development of the earlier mouth-to-ear technique through the advent of telephones, radio, and, above all, of the dictaphone.

Nor has Man limited his employment of language to establishing communication between himself and others of his kind. He has—not very successfully, it is true—tried it on his domesticated animals, and furthermore, whenever his fancy has created for him unseen creatures of another world, sprites and fairies, gods and demons, he has used language, in one of the ways mentioned, to attempt to get into touch with these also.

Finally he makes a further serious use of language in communicating, so to say, with himself, as every diligent note-taking student must admit, let alone those who realize that both thoughts and dreams are largely clothed in language.

Historically, grammar (and this is universally recognized) came into being as a consequence of the discovery of the art of writing. Like all human techniques writing must be taught—and learned. Now it seems to have been a universal law of human development that the possessors of technique, the more so if the technique was an intricate one, expected some recompense for their trouble in imparting it to others. The pedagogic function became in time a valuable means of obtaining a livelihood.

That specialist among pedagogues, the grammarian, was not slow to realize the advantage to himself of making his craft as difficult as he could. His influence is discernible on the development of all written languages, but outstandingly so on some of the Eastern ones such as Arabic and Chinese, where his hold has been so great that an almost unbridgeable gap has been created between the written and the spoken word.

It is at this point that revolt begins to mature. Human beings need language as a means of communication. If the pedant insists on making the thing too difficult his whole *raison d'être* is gone. A generation arises that will have none of it. *Ars longa vita brevis*. We have no time for your lengthy exercises. Language is for man, not man for language. Enough ! We shall write as we speak, and speak as we think.

The author's quotation on p. 104 from Samuel Butler is not the least entertaining part of the book, but he has surely let pass an occasion to chastise that subtle philosopher's logic by endorsing the argument that 'if a snuff-box can say "Send me a quart of beer" so efficiently that the beer is sent, it is impossible to say that it is not a *bona-fide* sentence!' Is a net, then, a *bona-fide* fishing-rod, because it can serve, like the rod, in the process of getting a Tay salmon into the mayonnaise ? We think it would be a risky line of defence to argue thus in the Sheriff's Court at Perth. N. B. HUNTER.

EVERYDAY THINGS IN ARCHAIC GREECE. *By* MARJORIE *and* C. H. B. QUENNELL. *Batsford*, 1931. *pp.* 142 *and index, illustrations and maps.* 7*s* 6*d*.

Again Mr and Mrs Quennell (and Messrs. Batsford) have given us a book which it is a pleasure to handle and to look at, but again we are left wishing that they would

confine themselves to the purpose set forth in the title, a consideration of *things* and illustrations of them. The historical introduction is confusing, and a summary of the whole text of Herodotus does not make it clearer. The style of writing is often careless, as page 33 ' different to ', page 12 ' gymnic ' (horrible word), and it seems peculiar in a book for children to say of the marital concerns of the Gods on Olympus that ' all these relationships were regularized by Hesiod ', while a digression on the inefficiency of modern teachers in preparing boys and girls for the School Certificate examination in mathematics is superfluous in a description of the Abacus.

It is difficult to feel sure for what aged child the book is intended. On the one hand we are told that ' the Pythoness was the medium between the God and the priests ' and that ' the modern man has been confined in a two-dimensional world ', neither very comprehensible statements for the young; while in other places the narrative is extremely simple.

But in spite of the colloquial and discursive style of much of the letterpress, we are sure that the delightful pictures and lucid explanations of such matters as smelting, shipbuilding, or the possible origin of the lyre from the shell of a tortoise, to take only a few examples, will make the Ancient World more real to many readers than it ever was before. And that is, after all, no small achievement. When so much is good it seems a pity that the rest should not be better. All school libraries will need this book for its illustrations.

DINA PORTWAY DOBSON.

ANCIENT CORINTH, with a topographical sketch of the Corinthia. Part I. From the earliest times to 404 B.C. *By* J. G. O'NEILL, PH.D. *Johns Hopkins University Studies in Archaeology. Number* 8. *Edited by* DAVID M. ROBINSON. *Oxford University Press. pp.* 270, 8 *illustrations, map and plan.* 22*s* 6*d.*

Professor O'Neill has set out to write a book in which ' the History of Greece, too often treated as if it were merely the history of Athens and Sparta would be surveyed with Corinth as the central figure '. To this admirably chosen task he has brought great learning and industry, and his work will be most valuable to scholars interested in the city on the isthmus. Unfortunately the book is rather spoiled for the general historical reader by the large number of quotations and references in the text to numerous authorities. We wish that the author would give us more of his own considered opinion and less of the raw ingredients of his dish. The topographical study of the district is detailed, and makes clear the writer's familiarity with the site, but the map is not attractive.

The subjects dealt with include prehistoric Corinth, early cults and myths, the Tyranny and succeeding constitutions, the Colonial System, and the Persian and Peloponnesian wars. The selected bibliography covers several pages. This volume should certainly find a place on the shelves of classical libraries. DINA PORTWAY DOBSON.

THE ROMAN VILLA AT CORINTH. *Prepared by the American School of Classical Studies at Athens.* Part V, *by* Professor THEODORE LESLIE SHEAR. 1930. *Harvard University Press*; *Otto Harrassowitz, Berlin.* Large portfolio. $10.

Corinth seems to be well to the fore as a subject of archaeological study at the present time, largely owing to the work and publications of the American School of Classical Studies at Athens. Amongst these, none can compete for magnificence with *The Roman Villa*, by Professor Shear. This work is published in portfolio form,

measuring 32 inches by 25, and its chief feature is a series of coloured plates giving facsimile reproductions of the fine pavements of the villa, made from paintings from the brush of the late Nora Jenkins Shear.

It is gathered that the villa was built and the pavements laid down in the Hellenistic period, so that the mosaics, representing a magnificent head of Dionysus, and several mythological and rustic scenes, throw some light on Greek painting. At a later date the floors were apparently used for a second Roman building, with rooms of such irregular shapes that the pavements had to be mutilated to fit them. The material for the mosaic was limestone, terra-cotta and glass, and the effect certainly is, both in the case of the figures and the geometrical patterns, above the usual Roman standard.

This publication includes an account of the building and a coloured reconstruction of the probable appearance of the façade of the house, and also ground plans, from the pencils of R. Stillwell and W. V. Cash. The undertaking was financed by Mr and Mrs Pierpont Morgan and Mr J. Patten, and we are left wishing that some of our English villa pavements, in many cases doomed to destruction, could be so splendidly commemorated

DINA PORTWAY DOBSON.

BULLETIN OF THE AMERICAN SCHOOL OF PREHISTORIC RESEARCH in affiliation with the Archaeological Institute of America. *Edited by* G. G. MACCURDY, *Director*. Nos. 6–8, 1930–2.

We have come to expect a high standard from the reports issued by the American School of Prehistoric Research under the direction of Grant MacCurdy, and the three numbers here reviewed form no exception.

Miss Garrod, in her paper 'The Palaeolithic of South Kurdistan: excavations in the caves of Zarzi and Hazar Merd' (no. 6), describes the results of a joint expedition sent out by the American School of Prehistoric Research and the Percy Sladen Memorial Fund, which was the first regular archaeological expedition to enter the Sulaimani district. At Zarzi the archaeological deposit rested on a terrace at the mouth of the cave. The implements were yielded by one layer from ½ to 1½ metres thick showing no natural subdivisions. Jaspers, flints, and cherts were the commonest materials used. The industry seems to belong to the Grimaldi facies of the Upper Aurignacian, being characterized by angle-gravers, Gravette points, and the single-shouldered point. In the upper levels a slight development is indicated by the appearance of microliths. Miss Garrod suggests that in time the industry at Zarzi should probably be placed at the end of the Upper Palaeolithic, giving us yet another example of an evolved Upper Aurignacian taking the place of the rather local Magdalenian.

At Hazar Merd the archaeological deposit was located inside the cave and not on a terrace as at Zarzi. The main interest concentrates on the rich Mousterian found in a layer from .5 to 3.9 metres thick, and sealed in by another from 1–2 metres thick containing objects dating from the Bronze Age to the present day. At the junction of these two deposits there occurred a thin wisp of Upper Palaeolithic, not more than from 1–2 cms. thick, but linked to the Zarzi industry by a typical single-shouldered point. The Mousterian of Hazar Merd agrees with that of Palestine in showing the following differences from the European :—(a) presence of gravers, (b) high proportion of narrow points and blades, (c) use of the flat retouch commonly. As Miss Garrod rightly says this can only be interpreted as indicating contact with Upper Palaeolithic culture ; this is just what we are now recognizing in Africa. The incomplete and unsatisfactory

faunal remains are dealt with by Miss D. M. A. Bate, who does however venture on other grounds to postulate a lower temperature for the local Mousterian. On the face of it the fact that Upper Palaeolithic man was able to live out on the terrace at Zarzi, while Mousterian man was compelled at Hazar Merd to seek shelter inside the cave, would certainly point to this conclusion.

Another paper by Miss Garrod describes the first two seasons' excavations in the caves of the Wady al-Mughara (no. 7), at the foot of the western slope of Mount Carmel. The second chamber of the cave known as Mugharet el-Wad contained the most complete sequence of archaeological deposits so far found in Palestine :—

A. Bronze Age to recent.
B. Mesolithic.
C. Upper Palaeolithic of Capsian affinities.
D. Middle Aurignacian.
E. Lower Middle Aurignacian.
F. Layer of erosion, containing both Aurignacian and Mousterian forms.
G. Mousterian. This was found only in one alcove where the floor went lower.

The Mousterian had already been found in Palestine, but the discovery of the Middle Aurignacian outside Europe is quite new, and provides yet another instance of how modern discovery in Africa and the Near East is tending to draw attention to the essentially parochial character of what passes for prehistory *par excellence* in Western Europe. The industry from layer ' C ' is at the moment without exact parallels though it has Capsian affinities. The Mesolithic industry, which was also found on the terrace outside the cave, includes numerous crescentic or lunate microliths, over 300 microburins, sickle-blades with corn-lustre, grooved hafts, bone points and pins, harpoons with a single row of barbs, bone pendants and beads, and two pieces of naturalistic art in the round. While the industry quite definitely shows a predominantly Mesolithic tradition, it also seems to show influences from more advanced cultures. The absence of pottery is not in itself a conclusive argument for regarding the industry as Mesolithic in *age* ; it is conceivable that tribes with Mesolithic traditions and way of life might well have persisted in certain areas borrowing from their more advanced neighbours on a strictly selective basis, accepting in this case the composite sickle idea—not necessarily for cultivated grasses or cereals—but rejecting the pottery. Such selective borrowing is a commonplace of anthropology. In fact we should hesitate to regard the industry from layer B of the Mugharet el-Wad as of Mesolithic *age*, though beyond a doubt the *tradition* is predominantly Mesolithic. The same industry was also discovered resting immediately upon the outside of the cave, but more work was done upon this in the succeeding season and we shall discuss it when we pass on to the next Bulletin.

Mr G. G. MacCurdy describes (no. 7) the result of seven seasons' work done by the American School in the Abri. The main deposits were of Mousterian age. Of special interest as rarities are seven rock-crystal implements, some of them exceedingly well made. MacCurdy justly remarks that the ratio of points to scrapers is far smaller in the European than in the Palestinian Mousterian. The Aurignacian was represented only poorly and locally belonging to a late phase of the culture.

In a third paper (no. 8) on the excavations in the Wady al-Mughara in 1931, Miss Garrod tells us more of the Mesolithic deposit from the terrace of the Mugharet el-Wad. She is able to sub-divide the deposit into an upper and a lower, the former of which corresponds to the Shukba Mesolithic industry, and the latter to that in the second

chamber of the cave itself as described in the previous bulletin. Sealed in by both was a remarkable platform of artificially prepared rock, the surface of which showed the marks of some kind of pick. In the middle of this platform was a rock-cut basin of v-shaped section some 30 cms. deep, while smaller basins were found outside the platform. Another curious discovery was that of a kind of crazy pavement of limestone slabs. Another feature of the work was the discovery of no less that 17 well-defined burials, mostly flexed and lying on one side, in addition to fragmentary indications of others. Three were of especial interest, one having a cap of bone beads or pendants, another with one of *dentalia*, and a third with a circlet of seven rows of *dentalia* still in place around the head. These burials occurred in both parts of the deposit. Miss Garrod has given the name Natufian to the culture after its first place of discovery, Shukba in the Wady en-Natuf.

Another cave in the Wady al-Mughara, namely Mugharet-el-Tabun, proved to contain a Mousterian industry associated, for the first time, with an abundant fauna. While the most northerly cave of the Wady, the Mugharet es-Sukhul excavated and reported on by T. D. McCown yielded in addition to a Mousterian industry the skeleton of a Neanderthal child in a remarkably good state of preservation.

J. G. D. CLARK.

THE EPHEMERIDES OF ALEXANDER'S EXPEDITION. *By* C. A. ROBINSON, JR. *Providence : Brown University*, 1932. *pp.* 81, 1 *plate and map*. 3 *dollars*.

This useful tabulation of the agreements and disagreements of the various authorities for the Expedition of Alexander to India are compiled to prove that our extant accounts were based on the daily Journals which are known to have been kept by two officers, Eumenes and Diodotus, while the expedition was in progress. In the course of the tabulation the important fact emerges that the extant authors seem to have been unable to use these Journals for a long period which corresponds to the journey from Bactra to the Hydaspes (including the northward push up to Chojend). The writer concludes that the Journals for this section had been lost. The actual occasion of the loss seems to have been when the tent of Eumenes was burned. From the Hydaspes to the final return, on the other hand, the Journals were accessible. This and other conclusions which depend upon it are important. The author of this slim volume has done a useful piece of work and helped to illuminate the numerous problems concerned with this greatest of all military expeditions.

S. CASSON.

THE SEARCH FOR MAN'S ANCESTORS. *By* Prof. G. ELLIOT-SMITH. No. 16 in the *Forum Series*. *London : Watts and Co.*, 1931. *pp.* 56. *Paper covers 7d, cloth 1s.*

The oncoming generation discards Adam and Eve soon after it has bidden farewell to Father Christmas ; and it demands, sooner or later, to be told what is actually known of the true origins of Man.

The fascinating story of those origins is admittedly, as yet, only fragmentary, but, such as it is, it is ably summarized in these pages by a high authority whose practical work is as well-known as his erudition.

No one who aims at building up for himself a synthetic philosophy in keeping with the needs of today, can afford to overlook *The Search for Man's Ancestors* ; the more so, since it performs a double service by giving us at once a handy, reliable book of reference and an excellent general survey.

QADESH: Mission Archéologique à Tell Nebi Mend, 1921–1922. *By* MAURICE PÉZARD. *Paris: Librairie Orientaliste Paul Geuthner*, 1931. *pp.* VII, 87, 3 *plans and* 46 *plates.* 200 *francs.*

The exploration of the important Hittite city of Kadesh, undertaken by M. Pézard in 1921 and the following year, was abruptly terminated by the excavator's untimely death in 1923. An 'interim' report had, however, already appeared from his pen in *Syria* (vol. III, 1922) and this, together with his unpublished manuscript notes on the campaign of 1922, constitutes the text of the present volume. A number of new photographs have also been added and there is a careful documentation of the material discoveries. Unfortunately the excavator had not yet arrived at that stage in his investigations when he could feel himself the master of a confused and baffling problem. The unravelling of the history of this great site still awaits the hand of an experienced archaeologist, and the preliminary soundings which were all that M. Pézard could accomplish, will then, without doubt, prove of considerable value. W. J. PHYTHIAN-ADAMS.

LA SYRIE. Antique et Médiévale illustrée. *By* R. DUSSAUD, P. DESCHAMPS, *and* H. SEYRIG. *Paris: Librairie Orientaliste Paul Geuthner*, 1931. *pp.* 22 *and* 160 *phototype plates.* 200 *francs.*

This is, in the truest sense, a superb production, more than worthy of taking its place as the seventeenth volume issued by the *Service des Antiquités et des Beaux Arts* of the French Mandatory Government in Syria and the Lebanon. The illustrations, which are magnificent specimens of photography, are introduced by M. Réné Dussaud, who focuses most felicitously into twenty pages the vast panorama of Syrian history. In addition to this, each plate has a page of explanation to itself and there is a useful index at the end. Every period receives adequate treatment from 2000 B.C. to the days of the Turkish domination. Of the art treasures illustrated, those of ancient Byblos are of special beauty, while the views of Palmyra and Baalbec are equally striking in their own field. There is a fine series of photographs of Crac des Chevaliers (Qal'at el Hosn) and here as elsewhere in the volume the aeroplane has played an indispensable part. Such a production as this will, we hope, stimulate to rivalry our own Department of Antiquities in Palestine. We have not there, it is true, the extraordinary wealth and variety which is afforded by the much larger territory of Northern Syria, yet it would not be difficult to fill a volume of this size with a series not much inferior in artistic and spectacular interest. W. J. PHYTHIAN-ADAMS.

ΠΡΟΪΣΤΟΡΙΚΗ ΕΛΕΥΣΙΣ ὑπὸ Γ. Ε. ΜΥΛΩΝΑ. *Published by the Archaeological Section of the Ministry of Education, Athens*, 1932. *pp.* 183 *and* 128 *figs. Price not stated.*

Dr Mylonàs has in this elaborate and well-illustrated paper published all the information about the prehistoric site which he has recently excavated at Eleusis. The site is an important one, since there is a complete series of cultures revealed from the early Helladic period to the latest phases of the Mycenaean. He has to show a particularly fine group of Minyan vessels, while the finds in painted wares of the First Late Minoan period are rich and important. The whole series of finds testifies to the continuous occupation of this site at Eleusis through all periods.

All the phases of mainland prehistory are here illustrated and useful comparisons can now be made between Eleusis and sites in the Peloponnese, where a similar sequence occurs.

The importance of this particular site is that it demonstrates the sort of life lived on the mainland before and after the advent of Cretan influences. There is no question

that, even after the arrival of Cretan trade and exploitation, these small villages lived in relative poverty and squalor. A bronze dagger and a stone lamp of typical Mycenaean make are the only objects which the inhabitants must have considered as things of value and wealth. Otherwise the village was a simple settlement of mainlanders in whose life the might of Tiryns and Mycenae counted for little.

The author holds unusual views on the origin of Minyan wares and doubts the views of those who derive some of the Minyan shapes from metallic prototypes in silver.

The site was still occupied in the ' Achaean ' period as is testified by some fine examples of vessels generally accepted as of this phase. Eleusis will, in fact, now take its place as a good type-site where the whole sequence of cultures can be studied. The author is to be congratulated on a careful and well-documented piece of work.

S. CASSON.

THE MUSEUM OF FAR EASTERN ANTIQUITIES, STOCKHOLM. Bulletin no. 3. *Stockholm*, 1931. *pp.* 176 *and* 17 *plates.* 20*s.*

A question of the utmost importance to Chinese archaeology is how far the *Chou Li*, with its numerous descriptions of ritual objects, can be regarded as an authentic pre-Han work. By arguments completely convincing, but too elaborate to summarize here, Bernhard Karlgren shows that though the book contains later interpolations, the greater part of it can be definitely proved to be pre-Han and ' can be freely used for archaeological researches ' (p. 59). These results are likely to be generally accepted. But the question still remains : does the *Chou Li* represent an actual state of affairs or merely one which the compilers imagined to have existed ?

An attempt is now being made to classify ancient Chinese bronzes geographically as well as chronologically. An important step in this direction is made by Olov Janse in his long article ' Un groupe de bronzes anciens propre à l'Extrême-Asie méridionale '. Two short articles, Waley's ' Magical use of phallic representations ; its late survival in China and Japan', and Erkes's ' Some remarks on Karlgren's "Fecundity symbols in Ancient China" ' are supplementary to an article by Karlgren in the last number of the Bulletin. Hanna Rydh supplies an extremely interesting article on ' Seasonal fertility rites and the death cult in Scandinavia and China '. ARTHUR WALEY.

ROCK-PAINTINGS AND OTHER ANTIQUITIES OF PREHISTORIC AND LATER TIMES. *By* Rai Sahib Manoranjan Ghosh. (Memoirs of the Archaeological Survey of India, no. 24). *Calcutta : Government of India*, 1932. *pp.* 125 *and* 28 *plates.* *Rs.* 13–6 (21*s* 9*d*).

This publication deals with prehistoric remains found near Chakradharpur, a station on the main line of the Bengal-Nagpur railway ; and rock-paintings at Singanpur, Mirzapur and Hoshangabad. Singanpur is on the same railway, close to the Chhota Nagpur border. Mirzapur is between Allahabad and Benares. Hoshangabad is the headquarters of the district of the same name in the Central Provinces.

The remains at Chakradharpur range from Palaeolithic to Iron Age. The paintings at Singanpur, representing animals and hunting-scenes, are palaeolithic in type ; there are other paintings of much later date. Those at Mirzapur range from the 4th to the 10th century A.D. ; those at Hoshangabad are 9th or 10th century. The paintings are not in caves but in rock-shelters. In each case stone implements were found in or near the shelters. The publication is an important one to the student of Indian art no less than to the specialist in prehistoric archaeology. ARTHUR WALEY.